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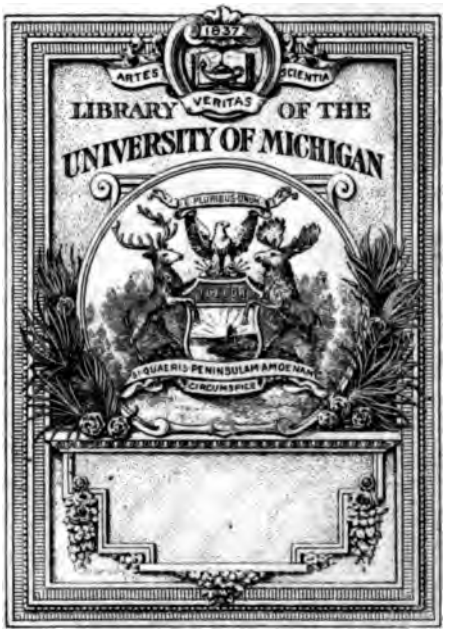
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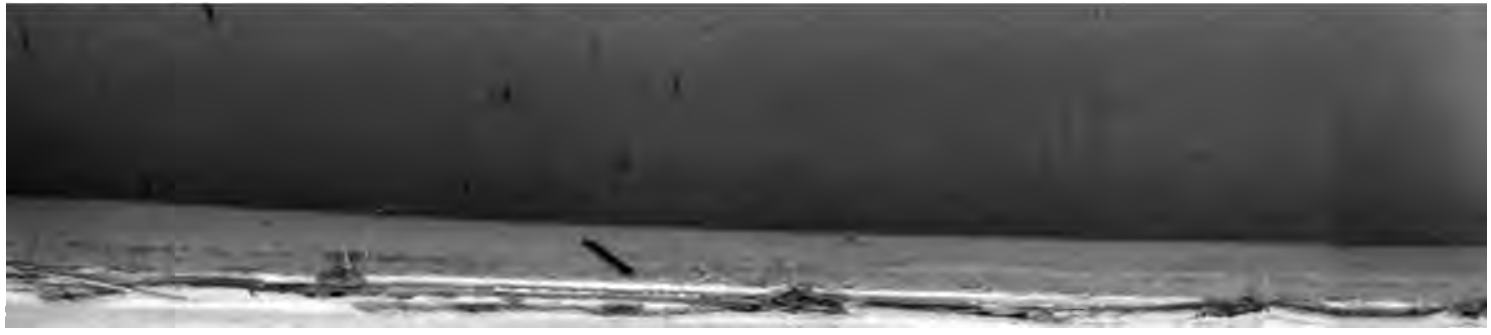

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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

THIRTY-FIRST MEETING,

HELD AT

Montreal, Canada,

AUGUST, 1882.

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TABLE OF CONTENTS.

	PAGE
Officers of the Montreal Meeting	ix
Standing Committee of the Montreal Meeting	x
Local Committee of Montreal Meeting	xi
Officers for the Minneapolis Meeting	xiii
Standing Committee for the Minneapolis Meeting	xiv
Local Committee for Minneapolis Meeting	xv
Special Committees of the Association	xvi
Meetings and Officers of the Association of American Geologists and Naturalists	xix
Meetings and Officers of the Association	xx
Act of Incorporation	xxii
Constitution of the Association	xxiii
Order of Proceedings in Organizing a Meeting	xxx
Members of the Association:	
Patrons	xxxi
Members	xxxi
Honorary Fellow	lxiv
Fellows	lxiv
Deceased Members	lxxviii

ADDRESS OF THE RETIRING PRESIDENT, GEORGE J. BRUSH	1
ADDRESS OF JAMES HALL, RETIRING PRESIDENT FOR THE FIRST MONTREAL MEETING, 1857	29

SECTION A. MATHEMATICS AND ASTRONOMY.

Address of Vice President, WM. HARKNESS	77
New views of Mr. George H. Darwin's theory of the evolution of the Earth-moon system, considered as to its bearing on the question of the duration of geological time. By SAMUEL HAUGHTON	89
The parallax of α Lyræ and 61 Cygni. By ASAPH HALL	93
On a method of reducing different catalogues of stars to a homogeneous system. By WILLIAM A. ROGERS	99
On the performance of a new form of level invented by Mr. John Clark of the U. S. Coast Survey. By WILLIAM A. ROGERS	102
On the arithmetic of chords. By C. P. HART	105

PAGE

Circular coördinates and complex anharmonic ratios. By WM. WOOLSEY JOHNSON 106
Inverse elliptic functions and the imaginary period. By WM. WOOLSEY JOHNSON 107
Conservation of solar energy. By PLINY EARLE CHASE 108
Correction of a problem in "The System of the World," by Sir Isaac Newton. By DE VOLSON WOOD 109
On an experimental solution of a problem in the doctrine of chances. By T. C. MENDENHALL 110
Bell attachment for telescope circles. By HENRY N. PARKHURST . . 111
The twenty-three inch telescope of the Halsted Observatory at Princeton. *Illustrated.* By C. A. YOUNG 112
On the law of distribution for certain plant-numbers. By JAMES EDWARD OLIVER 117
A method of finding the law of linear elasticity in a metal. By JAMES EDWARD OLIVER 118
A method of eliminating the personal equation in transit observations. By J. BURKITT WEBB 118
Note on a new method of graphically expressing any cyclical fact in meteorology. *Illustrated.* By W. E. HAMILTON 121
Titles of other papers read in the section 124

SECTION B. PHYSICS.

Address of Vice President, T. C. MENDENHALL 127
On the apparent size of magnified objects. By WM. H. BREWER . . 139
The spectroscopic rain-band. By WINSLOW UPTON 145
On the duration of color-impressions upon the retina. By E. L. NICHOLS . 146
On a mean-direction integrating anemometer. By CHARLES K. WEAD . 147
On vision by the light of the electric spark. By W. LE CONTE STEVENS . 148
The binocular union of spectral images. By W. LE CONTE STEVENS . 149
Upon the electrical experiments to determine the location of the bullet in the body of the late President Garfield; and upon a successful form of induction balance for the painless detection of metallic masses in the human body. *Illustrated.* By ALEXANDER GRAHAM BELL . . 151
On secondary batteries. *Illustrated.* By GEORGE F. BARKER . . 207
On modifications of the spectrum of sodium vapor in a Bunsen flame. *Illustrated.* By CHARLES S. HASTINGS 218
Siemens' electrical deep-sea thermometer. By J. R. BARTLETT . . 221
An instrument for readily producing low temperatures. By HENRY CARMICHAEL 223
Upon a proposed method of producing artificial respiration by means of a vacuum jacket. By ALEXANDER GRAHAM BELL . . . 224
Radiant heat an exception to the second law of thermodynamics. By H. T. EDDY 225
Titles of other papers read in the section 225

SECTION C. CHEMISTRY.

Address of Vice President, H. CARRINGTON BOLTON . . . 229
On a new microchemical method of determining the feldspars in rocks. By J. SZABÓ 257
Application of organic acids to the examination of minerals. Third paper. By H. CARRINGTON BOLTON 271

PAGE
Carbon dioxide as a constituent of the atmosphere. By ERNEST H. COOK . 276
A simple laboratory device. By ERNEST H. COOK . . . 281
The action of water at 100° C. on the phenyltribrompropionic acid. By LEONARD P. KINNICUTT and GEORGE M. PALMER 281
The action of baric hydrate on chlortribrompropionic acid. By C. F. MABERY and RALPH WILSON 282
On certain substituted acrylic and propionic acids. By C. F. MABERY and F. C. ROBINSON 283
On the products of the distillation of wood at low temperatures. By C. F. MABERY 284
Preliminary notice of a new organic base. By ALBERT R. LEEDS . . 284
Estimation of dextrose in solid commercial starch sugar by loss of rotatory power on solution. By HARVEY W. WILEY and C. A. CRAMPTON . 285
Direct estimation of dextrose, dextrine and maltose in commercial amylose (starch sugar). By HARVEY W. WILEY 285
On the constitution of benzole. By L. W. ANDREWS . . . 286
Some tea analyses, with special reference to the determination of caffeine. By W. HODGSON ELLIS 287
Pemberton's method for the volumetric determination of phosphoric acid. By G. C. CALDWELL 289
Titles of other papers read in the section 292

SECTION D. MECHANICAL SCIENCE.

Address of Vice President, W. P. TROWBRIDGE . . . 295
On a newly discovered absolute limit to economical expansion in the steam engine and in other heat motors. By ROBERT H. THURSTON . . 307
Transverse strength of large spruce beams. By GAETANO LANZA . 311
A method of cutting screws of increasing pitch. *Illustrated.* By J. BURKITT WEBB 314
Indicator attachment for high speeds. By J. BURKITT WEBB . . 316
Experiments to determine the strength of cylinders with domes attached. By SAMUEL MARSDEN 317
Aërial navigation. By JOS. DE L'ETOILE 318
The future of the balloon as a practical means of aërial travel. By W. H. LYNCH 319
Titles of other papers read in the section 321

SECTION E. GEOLOGY AND GEOGRAPHY.

The glacial flood of the Connecticut River valley. By C. H. HITCHCOCK . 325
Contribution to seismology. By RICHARD OWEN 329
Law of fracture, or fissuring, applied to inorganic and organic matter. By RICHARD OWEN 337
Subterranean map-making. *With maps.* By HORACE C. HOVEY . . 345
Deep-sea soundings and temperatures in the Gulf Stream off the Atlantic coast, taken under the direction of the U. S. coast and Geodetic survey. *With tables.* By J. R. BARTLETT 349
Preglacial channel of Eagle river, Michigan. By CHAS. WHITTLESEY . 352
Recent discoveries of fossil fishes in the Devonian rocks of Canada. By J. F. WHITEAVES 353
Note on the occurrence of Siphonotreta Scotica (Davidson) in the Utica formation near Ottawa, Ont. By J. F. WHITEAVES . . . 356

PAGE

On a post-tertiary deposit containing impressions of leaves, in Cumberland county, New Jersey. By N. L. BRITTON 357
Terraces and beaches about Lake Ontario. By J. W. SPENCER . . 359
Occurrence of Graptolites in the Niagara formation of Canada. By J. W. SPENCER 363
A Mastodon Americanus in a beaver meadow. By SAMUEL LOCKWOOD . 365
Currents of air and ocean in connection with climates, regions of summer rains and summer droughts. By J. BEAUFORT HURLBERT . . 367
A source of the bituminous matter of the Black-shales of Ohio. By EDWARD ORTON 373
Recent investigations and palæontological discoveries in the Wappinger limestone of Dutchess and neighboring counties, New York state. By WILLIAM B. DWIGHT 384
The Winooski marble of Vermont. By G. H. PERKINS 388
The great terminal moraine across Pennsylvania. *Illustrated.* By H. CARVILL LEWIS 389
The silicified stumps of Colorado. By ROBERT B. WARDER . . 398
The change of relative level of the ocean and the uplands on the eastern coast of North America. By GEORGE H. COOK 400
On the classification and origin of joint structures. By W. O. CROSBY . 409
The undulations of the rock-masses across central New York state. By HENRY S. WILLIAMS 412
Occurrence of magnetic ore deposits in Victoria county, Ontario. By W. HAMILTON MERRITT 413
Comparative view of the successive Palæozoic floras of Canada. By J. W. DAWSON 415
On the origin of joint cracks. By H. F. WALLING 417
On the association of crystals of quartz and calcite in parallel position. By R. B. HARE 419
Titles of other papers read in the section 419

SECTION F. BIOLOGY.

Address of Vice President, W. H. DALL 423
Variations in nature. A contribution to the doctrine of evolution, and the theory of natural selection. By THOMAS MEEHAN 437
Remarks concerning the flora of North America. By ASA GRAY . . 449
On the position of the Gamopetalæ. By LESTER F. WARD . . . 460
A revision of the genus Clematis of the United States. By JOSEPH F. JAMES 463
Achenial hairs and fibres of Compositæ. By G. MACLOSKIE . . 463
Some observations on the action of frost upon leaf-cells. By C. E. BESSEY 464
The motions of roots and radicles of Indian corn and of beans. By W. J. BEAL 466
Observations on the fertilization of Yucca and on structural and anatomical peculiarities in Pronuba and Prodoxus. By C. V. RILEY . . 467
The hibernation of Alctia xylina (Say) in the United States a settled fact. By C. V. RILEY 468
Emulsions of petroleum and their value as insecticides. By C. V. RILEY . 469
A new sexual character in the pupæ of some Lepidoptera. By J. A. LINTNER 470
On an egg-parasite of the currant saw-fly, Nematus ventricosus. By J. A. LINTNER 471
Observations on the elm-leaf beetle, Galeruca xanthomelæna. By G. MACLOSKIE 472
On the gall-mites, Phytoptus. By WM. A. BUCKHOUT . . . 473
On the mouth of the larva of Chrysopa. By WM. SAUNDERS . . 477

PAGE
The classification of the Ungulata. By EDWARD D. COPE . . 477
The fauna of the Puerco Eocene. By EDWARD D. COPE . . 479
On a recent species of Heteropora from the Strait of Juan de Fuca. By J. F. WHITEAVES 481
On the habits of Cryptobranchus. By BURT G. WILDER . . 482
The placental development in mammals: a unity of anatomical and physiological modality in all vertebrates. By HENRY O. MARCY . . 483
The morphology of arteries. By FRANK BAKER . . . 488
Illusions of motion. By H. P. BOWDITCH and G. STANLEY HALL . 489
Demonstration of a series of brains prepared by Giacomini's method. By WILLIAM OSLER 491
Cross heredity from sex to sex. By A. B. BLACKWELL . . . 492
The organic compounds in their relations to life. By LESTER F. WARD . 493
Classification of organisms. By LESTER F. WARD . . . 494
Titles of other papers read in the section 495

SECTION G. HISTOLOGY AND MICROSCOPY.

Address of Vice President, A. H. TUTTLE 499
Plant-"cells" and living matter. By LOUIS ELSBERG . . . 511
The third corpuscular element in the blood. By W. OSLER . . 511
The development of blood corpuscles in the bone-marrow. By W. OSLER . 513
Note on the microcytes in the blood and their probable origin. By W. OSLER 513
The histology of uterine fibroid tumors. Illustrated by a series of micro-photographs. By HENRY O. MARCY 513
Some vegetable poisons. By J. T. BURRILL 515
Notes on some of the structural and physiological peculiarities incident to the diseases of fruits. By D. P. PENHALLOW . . . 519
A new form of constant pressure injection apparatus. *Illustrated.* By WILLIAM LIBBEY, JR. 521
A filtering wash-bottle especially adapted to the use of the histologist. *Illustrated.* By C. E. HANAMAN 525
Development of the planula of Clava leptostyla, Ag. By J. H. PILLSBURY 527
Titles of other papers read in the section 528

ECTION H. ANTHROPOLOGY.

Address of Vice President, DANIEL WILSON 531
Notes on Mount Atlas and its traditions. By R. G. HALIBURTON . 559
Some hitherto unnoticed affinities between ancient customs in America and the other continents. By J. H. PHENÉ 569
The cross and the crucifix. By CHAS. WHITTLESEY . . . 573
Chief deities in American religions. By ALBERT S. GATSCHET . 573
Indian migrations as evidenced by language. By HORATIO HALE . 578
The kinship system and marriage laws of the Dhegiha, illustrated by charts. By J. OWEN DORSEY 579
The sun dance of the Ogalalla Sioux, By ALICE C. FLETCHER . . 580
The Davenport tablets. By HORATIO N. RUST 584
A find of ceremonial weapons in a Florida mound, with brief notice of other mounds in that state. *Illustrated.* By A. E. DOUGLASS . . 585
Recent archæological discoveries in Vermont. By G. H. PERKINS . 592
Titles of other papers read in the section 594

SECTION I. ECONOMIC SCIENCE AND STATISTICS.

	PAGE
Statistics in Agriculture. By J. R. DODGE	599
Notes upon some methods and results of collecting statistical matter by mail. By CHARLES W. SMILEY	601
Exhibition of some statistics of college men. By CHARLES W. SMILEY .	607
Some facts relative to the U. S. census of 1880. By CALVIN S. MIXTER .	611
Titles of other papers read in the section	612

EXECUTIVE PROCEEDINGS.

Report of the General Secretary	613
General Report of the Permanent Secretary	640
Cash Account of the Permanent Secretary	642

INDEX.

ERRATUM.

Page 10, line 16, for 1859 *read* 1858.

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OFFICERS ELECTED

FOR THE

MINNEAPOLIS MEETING.

THIRD WEDNESDAY IN AUGUST, 1883.

PRESIDENT.

C. A. YOUNG of Princeton.

VICE PRESIDENTS.

A. Mathematics and Astronomy—W. A. ROGERS of Cambridge.
B. Physics—H. A. ROWLAND of Baltimore.
C. Chemistry—EDWARD W. MORLEY of Cleveland.
D. Mechanical Science—DE VOLSON WOOP of Hoboken.
E. Geology and Geography—C. H. HITCHCOCK of Hanover.
F. Biology—W. J. BEAL of Lansing.
G. Histology and Microscopy—J. D. COX of Cincinnati.
H. Anthropology—O. T. MASON of Washington.
I. Economic Science, and Statistics—F. B. HOUGH of Lowville.

PERMANENT SECRETARY.

F. W. PUTNAM, of Cambridge.

GENERAL SECRETARY.

J. R. EASTMAN of Washington.

ASSISTANT GENERAL SECRETARY.

ALFRED SPRINGER of Cincinnati.

SECRETARIES OF THE SECTIONS.

A. Mathematics and Astronomy—W. W. JOHNSON of Annapolis.
B. Physics—C. K. WEAD of Ann Arbor.
C. Chemistry—J. W. LANGLEY of Ann Arbor.
D. Mechanical Science—[Resigned.]
E. Geology and Geography—ALEXIS A. JULIEN of New York.
F. Biology—S. A. FORBES of Normal.
G. Histology and Microscopy—CARL SEILER of Philadelphia.
H. Anthropology—G. H. PERKINS of Burlington.
I. Economic Science, and Statistics—JOSEPH CUMMINGS of Evanston.

TREASURER.

WILLIAM LILLY of Mauch Chunk.

MEMBERS OF THE STANDING COMMITTEE

FOR THE

MINNEAPOLIS MEETING.

Past Presidents.—JAMES D. DANA of New Haven; JAMES HALL of Albany; ISAAC LEA of Philadelphia; F. A. P. BARNARD of New York; J. S. NEWBERRY of New York; B. A. GOULD of Boston; T. STERRY HUNT of Montreal; ASA GRAY of Cambridge; J. LAWRENCE SMITH of Louisville; JOSEPH LOVERING of Cambridge; JOHN L. LECONTE of Philadelphia; J. E. HILGARD of Washington; SIMON NEWCOMB of Washington; O. C. MARSH of New Haven; GEORGE F. BARKER of Philadelphia; GEORGE J. BRUSH of New Haven; J. W. DAWSON of Montreal.

Vice Presidents of the last Meeting.—WM. HARKNESS of Washington; T. C. MENDENHALL of Columbus; H. C. BOLTON of Hartford; W. P. TROWBRIDGE of New Haven; E. T. COX of San Francisco; W. H. DALL of Washington; A. H. TUTTLE of Columbus; DANIEL WILSON of Toronto; ALEXANDER WINCHELL of Ann Arbor; E. B. ELLIOTT of Washington.

Officers of the Meeting.—C. A. YOUNG of Princeton; W. A. ROGERS of Cambridge; H. A. ROWLAND of Baltimore; EDWARD W. MORLEY of Cleveland; DE VOLSON WOOD of Hoboken; C. H. HITCHCOCK of Hanover; W. J. BEAL of Lansing; J. D. COX of Cincinnati; O. T. MASON of Washington; F. B. HOUGH of Lowville; F. W. PUTNAM of Cambridge; J. R. EASTMAN of Washington; ALFRED SPRINGER of Cincinnati; W. W. JOHNSON of Annapolis; C. K. WEAD of Ann Arbor; J. W. LANGLEY of Ann Arbor; ALEXIS A. JULIEN of New York; S. A. FORBES of Normal; CARL SEILER of Philadelphia; G. H. PERKINS of Burlington; JOSEPH CUMMINGS of Evanston; WILLIAM LILLY of Maunch Chunk.

From the Association at large.—One Fellow to be elected from each Section.

LOCAL COMMITTEE

FOR THE

MINNEAPOLIS MEETING.

CHAIRMAN.
Hon. GEORGE A. PILLSBURY.

SECRETARY.
Prof. N. H. WINCHELL.

TREASURER.
Hon. GEORGE A. PILLSBURY.

SUBCOMMITTEE ON INVITATIONS AND RECEPTION.
Chairman.—President W. W. FOLWELL.

SUBCOMMITTEE ON FINANCE.
Chairman.—J. C. SEELEY, Esq.

SUBCOMMITTEE ON TRANSPORTATION AND EXCURSIONS.
Chairman.—THOMAS LOWRY, Esq.

SUBCOMMITTEE ON ENTERTAINMENT, HOTELS, LODGINGS AND LUNCHEONS.
Chairman.—Hon. A. C. RAND.

SUBCOMMITTEE ON ROOMS AND PLACES OF MEETING.
Chairman.—Hon. EUGENE M. WILSON.

SUBCOMMITTEE ON PRINTING.
Chairman.— DAVID BLAKELY, Esq.

GENERAL COMMITTEE.

Hon. GEORGE A. BRACKETT; Hon. A. C. RAND; Hon. JOHN DE LAITTRE; Ex-Gov. JOHN S. PILLSBURY; W. W. MCNAIR, Esq.; Dr. WM. W. FOLWELL; CHARLES W. JOHNSON; Gen. A. B. NETTLETON; Hon. W. W. WASHBURN; T. B. WALKER; Hon. O. C. MERRIMAN; E. V. WHITE; Hon. H. T. WELLES; Col. H. G. HICKS; THOMAS LOWRY, Esq.; WINTHROP YOUNG; Hon. W. S. KING; DAVID BLAKELY; Hon. R. B. LANGDON; Hon. D. L. KIEHLE; J. C. SEELEY, Esq; ANTHONY KELLY; Dr. A. F. ELLIOTT; Hon. F. W. BROOKS; Hon. EUGENE M. WILSON.

SPECIAL COMMITTEES.

1. *Permanent Committee on Weights, Measures and Coinage.*

F. A. P. BARNARD, of New York,
B. A. GOULD, of Boston,
J. E. HILGARD, of Washington,
H. A. NEWTON, of New Haven,
J. LAWRENCE SMITH, of Louisville,
E. B. ELLIOTT, of Washington,
JOHN TROWBRIDGE, of Cambridge,
A. M. MAYER, of Hoboken.

2. *Committee to Memorialize the Legislature of New York for a New Survey of Niagara Falls.*

G. W. HOLLEY, of Niagara Falls,
J. T. GARDNER, of Albany.

3. *Committee on Membership.*

THE MEMBERS OF THE STANDING COMMITTEE.

4. *Committee on the best methods of Science Teaching in the Public Schools.*

E. L. YOUMANS, of New York,
A. R. GROTE, of New York,
J. S. NEWBERRY, of New York,
J. W. POWELL, of Washington,
N. S. SHALER, of Cambridge,
N. T. LUPTON, of Nashville.

5. *Committee on the Registration of Deaths, Births, and Marriages.*

E. B. ELLIOTT, of Washington,
J. B. KILLEBREW, of Nashville,
E. T. COX, of San Francisco,
F. B. HOUGH, of Lowville,
J. S. COPES, of New Orleans,
JOHN COLLETT, of Indianapolis.

6. *Committee on Standard Time.*

ORMOND STONE, of Univ. of Va.	LEWIS BOSS, of Albany,
S. P. LANGLEY, of Allegheny,	LEONARD WALDO, of New Haven,
E. C. PICKERING, of Cambridge,	J. K. REES, of St. Louis,
J. R. EASTMAN, of Washington,	G. W. HOUGH, of Lowville,

H. S. PRITCHETT, of Glasgow.

7. *Committee on Stellar Magnitudes.*

E. C. PICKERING, of Cambridge,	E. S. HOLDEN, of Madison,
LEWIS BOSS, of Albany,	SIMON NEWCOMB, of Washington,
S. W. BURNHAM, of Chicago,	C. H. F. PETERS, of Clinton,
ASAPH HALL, of Washington,	ORMOND STONE, of Univ. of Va.
WILLIAM HARKNESS, of Washington,	C. A. YOUNG, of Princeton.

8. *Committee on State Geological Surveys.*

G. C. SWALLOW, of Columbia,	N. H. WINCHELL, of Minneapolis,
JAMES HALL, of Albany,	W. C. KERR, of Raleigh,
JOHN R. PROCTER, of Frankfort,	J. W. POWELL, of Washington,

EDWARD ORTON, of Columbus.

9. *Committee to confer with Committees of Foreign Associations for the Advancement of Science with reference to an International Convention of Scientific Associations.*

T. STERRY HUNT, of Montreal, | ALEXANDER AGASSIZ, of Cambridge.
SIMON NEWCOMB, of Washington.

10. *Committee to confer with a Committee of the British Association for the Advancement of Science on the Annual Records of Science.*

J. W. DAWSON, of Montreal,	EDWARD S. DANA, of New Haven,
F. W. CLARK, of Washington,	H. W. MARTIN, of Baltimore,

C. A. WHITE, of Washington.

11. *Committee to devise a plan for the proper indexing of the Literature of the Chemical Elements.*

H. CARRINGTON BOLTON, of Hartford,
IRA REMSEN, of Baltimore,
F. W. CLARK, of Washington,
A. R. LEEDS, of Hoboken,
A. A. JULIEN, of New York.

12. *Committee on a Primary Meridian and an International Standard of Time.*

F. A. P. BARNARD, of New York,
C. A. YOUNG, of Princeton,
T. STERRY HUNT, of Montreal,
J. R. EASTMAN, of Washington,
CHARLES CARPMAEL, of Toronto,
E. B. ELLIOTT, of Washington.

13. *Committee on International Congress of Geologists.*

JAMES HALL, of Albany,
J. W. DAWSON, of Montreal,
J. S. NEWBERRY, of New York,
T. STERRY HUNT, of Montreal,
C. H. HITCHCOCK, of Hanover,
RAPHAEL PUMPELLY, of Owego,
J. P. LESLEY, of Philadelphia.

14. *Committee to confer with the United States Geologist in regard to coöperation between the Government and State Geological Surveys.*

JAMES HALL, of Albany,
T. STERRY HUNT, of Montreal,
J. S. NEWBERRY, of New York,
N. H. WINCHELL, of Minneapolis,
C. H. HITCHCOCK, of Hanover,
J. P. LESLEY, of Philadelphia,
G. H. COOK, of Brunswick,
E. A. SMITH, of Tuscaloosa,
JOSEPH LE CONTE, of San Francisco.

This Committee has power to add to its number.

15. *Auditors.*

HENRY WHEATLAND, of Salem,
THOMAS MEEHAN, of Germantown.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr., C. B. Trego,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,*	J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,*		J. S. St. John,* 7	S. F. Baird,	A. L. Elwyn.
8th	April 23 1854,	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte. 8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,*		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857,	Montreal, Canada,	Alexis Caswell,* 9	Alexis Caswell,*	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,*	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam.	W. S. Vaux.†

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION OF CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION OF MICROSCOPY.	CHAIRMAN OF PERMANENT SUBSECTION OF ENTOMOLOGY.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,*	———	———
25th	Aug. 23, 1876.	Buffalo, N. Y.,	W. B. Rogers,	C. A. Young,	E. S. Morse,	G. F. Barker,	L. H. Morgan,*	R. H. Ward,	———
26th	Aug. 29, 1877.	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,†	R. H. Ward,	———
27th	Aug. 21, 1878.	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	———[2]	R. H. Ward,[3]	———
28th	Aug. 27, 1879.	Saratoga, N. Y.,	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,	———
29th	Aug. 25, 1880.	Boston, Mass.,	L. H. Morgan,*	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,	———
30th	Aug. 17, 1881.	Cincinnati, Ohio,	G. J. Brush,	Wm. Harkness,[7]	E. T. Cox,[8]	G. C. Caldwell,[9]	G. Mallery,	A. B. Hervey,	J. G. Morris.

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.	SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.	TREASURER.
F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	———	———	W. S. Vaux.
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	———	W. S. Vaux.
F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	———[2]	T. O. Summers, jr.,	———	W. S. Vaux.†
F. W. Putnam,	H. C. Bolton,	F. E. Nipher,	George Little,	A. P. S. Stuart,	———[2]	C. J. Engelmann,	———	W. S. Vaux.†
F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.†
F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux
F. W. Putnam,	C. V. Riley,	E. T. Tappan,[10]	Wm. Saunders,	A. Springer,[12]	J. G. Henderson,	W. H. Seaman,[11]	B. P. Mann,	W. S. Vaux.†

[1] In the absence of E. C. Pickering.
[2] The Subsection united with Sec. B.
[3] In the absence of G. S. Blackie.
[4] In the absence of Ira Remsen.
[5] In the absence of George Little.
[6] In the absence of A. C. Wetherby.
[7] In the absence of A. M. Mayer.
[8] In the absence of George Engelmann.
[9] In the absence of W. R. Nichols.
[10] In the absence of John Trowbridge.
[11] In the absence of S. P. Sharples.
[12] In place of H. W. Wiley, called away.
* Deceased.
† Not present.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in General Session.

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in General Session.†

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Stand-

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*As amended at the Cincinnati Meeting, August, 1881, and to take effect at the Montreal Meeting, August, 1882.

†When this article was adopted in 1874 the following clause was added, but was stricken out as inoperative when the constitution was amended in 1881:—

"But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875."

ing Committee and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, an Assistant General Secretary, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Standing Committee shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the

Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee.

ART. 13. The Assistant General Secretary shall be the Secretary of the Standing Committee. He shall give to the Secretary of each Section the titles of papers assigned to it by the Standing Committee.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ one or more clerks at such compensation as may be agreed upon by the Standing Committee.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Assistant General Secretary, and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Standing Committee shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Assistant General Secretary, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A. M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The Committee shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other

proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; nominate members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association.

ART. 19. The Nominating Committee shall consist of the Standing Committee, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 21. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into nine Sections, namely:—A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to agriculture and the arts;* D, *Mechanical Science;* E, *Geology and Geography;* F, *Biology;* G, *Histology and Microscopy;* H, *Anthropology;* I, *Economic Science and Statistics.* The Standing Committee shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section nor Subsection until it has been received from the Standing Committee and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Standing Committee with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

PRINTED PROCEEDINGS.

ART. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles taken from the "title blanks" will appear in the printed volume. The Standing Committee shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

Admission Fee and Assessments.

Art. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 35. The annual assessment for members and fellows shall be three dollars.

Art. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Standing Committee.

Art. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

Art. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Standing Committee.

Alterations of the Constitution.

Art. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers entered and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Election of members.
7. Election of fellows.
8. Unenumerated business.
9. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28).

MEMBERS.[3]

Abbot, S. L., M. D., 90 Mt. Vernon St., Boston, Mass. (29).
Abbott, Dr. Chas. C., Trenton, N. J. (29).
Abbott, E. K., M. D., Salinas, Monterey Co., Cal. (29).
Abert, Col. James W., Newport, Ky. (31).
Abert, S. Thayer, 1907 Pennsylvania Avenue, Washington, D. C. (30).
Acker, E. O'C., Ass't Eng'r, U. S. N., Norristown, Pa. (28).
Adams, Frank, 41 McGill College Avenue, Montreal, Can. (29).
Adams, John S., M. D., Oakland, Cal. (29).
Adams, Julius W., 155 Congress St., Brooklyn, N. Y. (29).
Adams, Capt. Robert C., Montreal, Can. (31).
Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Allen, Edmund T., 404 Market St., St. Louis, Mo. (27).
Allen, Dr. G. M., 102 Clark St., Cincinnati, Ohio (30).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. The payment of ten dollars at one time entitles a member to the subsequent volumes bound in cloth, or by the payment of twenty dollars, to the volumes bound in half morocco.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

The names of Patrons are to remain permanently on the list.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Allen, J. M., Hartford, Conn. (22).
Alling, J. C., Supt. City Schools, Albert Lea, Minn. (31).
Alling, Miss Mary R., Acra, Greene Co. N. Y. (29).
Allis, Solon M., Tucson, Arizona (29).
Ammen, Daniel, Rear Admiral U. S. Navy, Washington, D. C. (26).
Anderson, Rev. Joseph, Waterbury, Conn. (29).
Anderson, Newton M., Columbus, Ohio (30).
Andrews, L. W., Ph. D., Springfield, Mass. (31).
Angus, William, 214 St. James St., Montreal, Can. (31).
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Archambault, U. E., Montreal, Can. (31).
Armstrong, Lucius H., St. Nicholas, Duval Co., Fla. (30).
Armstrong, Mrs. Lucius H., St. Nicholas, Duval Co., Fla. (30).
Arnold, Lauren B., Rochester, N. Y. (30).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Ashmead, Wm. H., Jacksonville, Fla. (29).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwater, Richard M., Millville, N. J. (29).
Atwood, E. S., East Orange, N. J. (29).
Atwood, Oscar, Rutland, Vt. (31).
Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29).
Aughey, Prof. Samuel, Univ. of Nebraska, Lincoln, Neb. (29).
Austin, Miss Mary, Mattapan Station, Milton, Mass. (31).
Ayres, Prof. Brown, Univ. of Louisiana, New Orleans, La. (31).
Ayres, Dr. S. C., 64 W. 7th St., Cincinnati, Ohio (30).

Babbit, John, Fredericton, N. B. (29).
Bailey, E. H. S., Bethlehem, Pa. (25).
Baker, A. L., 32 So. Charles St., Baltimore, Md. (30).
Baker, B. N., 32 So. Charles St., Baltimore, Md. (30).
Baker, Frank, M. D., 366 C St., Washington, D. C. (31).
Baker, Prof. I. O., Illinois Industrial Univ., Champaign, Ill. (30).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Baker, Wm. E., 158 Tremont St., Boston, Mass. (29). [(31).
Baldwin, Miss Mary A., Summer Ave., Cor. Kearney St., Newark, N. J.
Baldwin, Very Rev. Dean Maurice S., Montreal, Can. (31).
Balen, Abraham D., Plainfield, N. J. (31).
Ballard, H. H., Lenox, Mass. (31).
Ballou, Nahum E., M. D., Sandwich, Ill. (29).
Ballou, Wm. H., 105 Dearborn Avenue, Chicago, Ill. (29).
Banes, Charles H., 2416 Spring Garden St., Philadelphia, Pa. (31).
Baptie, George, Normal School, Ottawa, Ontario, Can. (29).
Barclay, Robert, A. B., St. Louis, Mo. (30).
Barker, Martha M., care of H. R. Barker, Steam and Gas Fixtures Lowell, Mass. (31).

Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., Hotel Vendôme, Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barrows, Nathan, M. D., Hartford, Conn. (29).
Bartlett, Arthur K., Battle Creek, Mich. (29).
Bartlett, Prof. Edwin J., Dartmouth Coll., Hanover, N. H. (28).
Bartlett, Dr. Homer L., Flatbush, L. I. (28).
Bassett, Daniel A., Crawfordsville, Ind. (29).
Bassett, George W., M.D., Vandalia, Ill. (20).
Bassnett, Thomas, Box 936, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Box 936, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Bastin, Prof. E. S., Univ. of Chicago, Chicago, Ill. (29).
Batchelor, Charles, Electrician, Menlo Park, N. J. (28).
Batterson, J. G., Hartford, Conn.(23).
Bausch, Edward, Rochester, N. Y. (26).
Bayne, Prof. Herbert A., Royal Military College, Kingston, Ont. (29).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beadle, Delos W., St. Catharines, Ontario, Can. (31).
Beal, F. E. L., Agricultural College, Ames, Iowa (29).
Beall, Thomas, Lindsay, Ontario, Can. (31).
Beamer, Miles (30).
Bean, Thos. E., Box 441, Galena, Ill. (28).
Beaudry, J. Alphonse U., 97 St. James St., Montreal, Can. (31).
Beauregard, Gen. Gustave T., New Orleans, La. (30). [(29).
Beckwith, Henry C., Passed Ass't Eng'r U. S. N., Coleman's Station, N. Y.
Beebe, Wm. S., Brevet Major, U. S. Army, 25 Grace Court, Brooklyn, N. Y. (30).
Belknap, Chas., Lieut. U. S. N., U. S. S. Vandalia, care Navy Dept., Washington, D. C. (29).
Belknap, Morris B., Louisville, Ky. (29).
Belknap, Wm. B., Louisville, Ky. (29).
Bell, Alex. Melville, 88 Fayette St., Georgetown, D. C. (31).
Bell, John J., Exeter, N. H. (22).
Bell, Thomas D., McGill College, Montreal, Can. (31).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y.
Bemrose, Joseph, 158 P. O., Montreal, Can. (31). [(25).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19).
Benjamin, Marcus, 43 East 67th St., New York, N. Y. (27).
Berliner, Emile, 95 Milk St., Boston, Mass. (29).
Best, W. F., St. John, N. B. (29).
Beyer, H. G., Passed Ass't Surgeon, U. S. N., 125 Park Ave., Baltimore, Md. (31).
Bickmore, Mrs. Albert S., 12 East 41st St,, New York, N. Y. (31).

Bicknell, Hon. Thos. W., Boston, Mass. (29).
Bigelow, Otis, 605 7th St., Washington, D. C. (30).
Bigelow, Wm. Sturgis, M. D., 61 Beacon St., Boston, Mass. (29).
Bill, Charles, Springfield, Mass. (17).
Birge, Charles P., Keokuk, Iowa (29).
Blackford, Eugene G., 809 Bedford Avenue, Brooklyn, N. Y. (29).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Blackwell, Mrs. A. B., Somerville, N. J. (30).
Blaisdell, F. E., Poway, San Diego Co., Cal. (29).
Blake, Mrs. Clarence J., 19 St. James Avenue, Boston, Mass. (29).
Blake, Francis C., Penna. Lead Works, Mansfield Valley, Alleghany Co., Pa. (29).
Blake, Prof. John R., Davidson College, N. C. (29).
Blakslee, Prof. T. M., Harpersville, N. Y. (31).
Blanchard, Frederick, Box CC, Lowell, Mass. (29).
Blatchford, Eliphalet W., 375 North La Salle St., Chicago, Ill. (17).
Blatchford, John S., 13 Exchange St., Boston, Mass. (29).
Blodgett, A. N., M.D., Boylston St., Boston, Mass. (29).
Boardman, Miss E. D., 120 Beacon St., Boston, Mass. (29).
Boardman, Mrs. Wm. D., 38 Kenilworth St., Roxbury, Mass. (28).
Bobbitt, Mrs. John, Greensburg, Decatur Co., Ind. (30).
Boerner, Chas. G., Vevay, Ind. (29).
Bolles, Dr. Wm. P., 571 Dudley St., Dorchester, Mass. (29).
Bond, Geo. W., Boston, Mass. (29).
Bookwalter, J. W., Springfield, Ohio (30).
Bourland, Dr. Addison M., Van Buren, Ark. (29).
Bowditch, Frederic C., Box 615, Brookline, Mass. (29).
Bowers, Miss Virginia K., care Wm. Russell, Memphis Appeal Office, Memphis, Tenn. (27).
Bowker, W. H., 43 Chatham St., Boston, Mass. (30).
Bowles, George J., 1466 St. Catherine St., Montreal, Can. (31).
Bowles, Miss Margaretta, The Institute, Columbia, Tenn. (26).
Boyd, William, Cambridge, Mass. (29).
Boynton, Miss S. P., Box 150, Lynn, Mass. (19).
Brackett, John W., 581 Washington St., Boston, Mass. (29).
Brackett, Solomon H., St. Johnsbury, Vt. (29).
Bradford, Edward H., M. D., 150 Boylston St., Boston, Mass. (29).
Bradford, Royal B., Lt. Comd'r, U. S. N., Torpedo Station, Newport, R. I. (31).
Bradley, C. W., cor. 3d and Walnut Sts., Cincinnati, Ohio (27).
Bragg, Everett B., No. 3 Park Place, New York, N. Y. (29).
Brainerd, Thos. C., P. O. Box 720, Montreal, Can. (31).
Brandegee, T. S., Berlin, Conn. (29).
Braulette, Mrs. T. E., Louisville, Ky. (29).
Branstrup, Dr. W. T., Vincennes, Ind. (30).
Bray, Prof. C. D., College Hill, Mass. (29).
Bray, Mrs. Maria H., West Gloucester, Mass. (29).

Breckinridge, S. M., Box 2794, St. Louis, Mo. (27).
Brewster, William, 61 Sparks St., Cambridge, Mass. (29).
Brissette, Milton H., Montreal, Can. (31).
Bronnson, W. H., Lt. Comd'r U. S. N., U. S. C. and G. Survey, St'r "Blake," Navy Yard, New York, N. Y. (31).
Bronson, E. H., Ottawa, Ontario, Can. (31).
Brooks, Miss Margarette W., Salem, Mass. (29).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brounell, Prof. Walter A., 725 Irving St., Syracuse, N. Y. (30).
Brown, Miss Anne, 528 West 7th St., Cincinnati, Ohio (31).
Brown, Arthur, C. E., Oakland, Alameda Co., Cal. (31).
Brown, Asa Warren, Newport, Ky. (30).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brown, Prof. C. J., Randolph, N. Y. (31).
Brown, Fred. G., care of McKesson and Robbins, 93 Fulton St., New York, N. Y. (31).
Brown, Hon. John C., Vice President, Texas and Pacific R. R. Co., Pulaski, Tenn. (26).
Brown, Jonathan, 390 Broadway, Somerville, Mass. (29).
Brühl, Gustav, N. W. cor. John and Hopkins Sts., Cincinnati, Ohio (28).
Bryant, Wm. S., 61 Beacon St., Boston, Mass. (30).
Buck, C. Elton, Wilmington, Del. (29).
Buck, Henry C., West Somerville, Mass. (29).
Buckingham, Chas. L., U. S. Patent Office, Washington, D. C. (28).
Buffum, Miss Fannie A., Linden, Mass. (29).
Bull, John, Room 30, Corcoran Building, Washington, D. C. (31).
Bulloch, Walter H., 99 W. Monro St., Chicago, Ill. (30).
Burdick, Edson A., 408 Spruce St., Le Droit Park, Washington, D. C. (30).
Burke, Jos. C., Middletown, Conn. (29).
Burke, William, U. S. Patent Office, Washington, D. C. (28).
Burland, Jeffrey H., Montreal, Can. (31).
Burpee, J. P. C., St. John, N. B. (30).
Burr, Prof. William H., Troy, N. Y. (31).
Bush, Rev. Stephen, D. D., Waterford, N. Y. (19).
Butcher, Mrs. Z. D., Washington, D. C. (29).
Butler, A. W., Brookville, Franklin Co., Ind. (30).
Butler, Cyrus, 24 Cliff St., New York, N. Y. (29).
Bynner, Edwin L., 13 Pemberton Square, Boston, Mass. (29).
Byrnes, R. M., M. D., cor. 5th and Race Sts., Cincinnati, Ohio (30).

Cabot, Sam'l, jr., 53 Kilby St., Boston, Mass. (29).
Calder, Edwin E., Box 17, Providence, R. I. (29).
Caldwell, Albert G., Lt. Comd'r U. S. N., Torpedo Station, Newport, R. I. (31).
Caldwell, Dr. Frank, Cincinnati Hospital, Cincinnati, Ohio (30).
Calkins, Dr. Marshall, Springfield, Mass. (29).

Campbell, Rev. Prof. John, Presbyterian College, Montreal, Can. (31).
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25).
Capen, Miss Bessie T., Northampton, Mass. (23).
Cargill, H. L., St. Lawrence Hotel, Montreal, Can. (31).
Carman, Ezra A., Dept. of Agric., Washington, D. C. (30).
Carman, Lewis, Bangall, N. Y. (29).
Caron, C. K., Louisville, Ky. (30).
Carpenter, Geo. O., jr., care St. Louis Lead & Oil Co., St. Louis, Mo. (29).
Carpmael, Charles, Director of Magnetic Observatory, Toronto, Can. (31).
Carrington, Col. Henry B., U. S. A., care Adj. Gen. U. S. A., Washington, D.C. (20).
Case, Col. Theo. S., Ed. Western Review of Science, Kansas City, Mo. (27).
Cassino, S. E., Naturalists' Agency, 41 Arch St., Boston, Mass. (25).
Castle, Frederick A., M. D., 102 East 57th St., New York, N. Y. (29).
Castner, Dr. H. Y., 197 Pearl St., New York, N. Y. (30).
Caswell, John H., 11 West 48th Street, New York, N. Y. (26).
Chabert, L'Abbé Joseph, 80 St. Gabriel St., Montreal, Can. (31).
Chace, Arnold B., Valley Falls, R. I. (29).
Chadbourn, Erlon R., Lewiston, Me. (29).
Chadwick, James R., M. D., Clarendon St., Boston, Mass. (29).
Chance, H. Martyn, 2433 Fairmount Avenue, Philadelphia, Pa. (29).
Chandler, Prof. Chas. Henry, Ripon, Wis. (28).
Channing, Dr. Wm. F., Providence, R. I. (29).
Chapman, F. M., Danforth, Iroquois Co., Ill. (17).
Chapman, Henry C., M. D., 1226 Walnut St., Philadelphia, Pa. (29).
Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26).
Chase, Prof. F. A., Fisk University, Nashville, Tenn. (26).
Chase, Mrs. Marine J., 1622 Locust St., Philadelphia, Pa. (31).
Chase, Prof. Pliny E., Haverford College, Haverford, Pa. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).
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Williams, Dr. E., 64 W. 7th St., Cincinnati, Ohio (30).
Williams, E. B., Strawberry Point, Iowa (24).
Williams, Prof. Edward H., jr., Box 463, Bethlehem, Pa. (25).
Williams, F. H., M. D., 100 Boylston St., Boston, Mass. (29).
Williams, Frank, Buffalo, N. Y. (25).
Williams, G. Henry, Baltimore, Md. (30).
Williams, J. Francis, Salem, N. Y. (31).
Williams, Janet W., Newton Lower Falls, Mass. (31).
Williams, P. O., M.D., Watertown, Jefferson Co., N. Y. (24).
Williams, Rev. R. R., Canajoharie, Montgomery Co., N. Y. (28).
Willoughby, Hugh L., Saratoga Springs, N. Y. (28).
Wills, William R., Waltham, Mass. (30).
Willson, Robert W., Box 853, New Haven, Conn. (30).
Wilmot, Thos. J., Direct U. S. Cable Co., Rye, N. H. (27).
Wilson, C. H., Rugby, Tenn. (30).
Wilson, H. C., Mt. Lookout, Hamilton Co., Ohio (30).
Wilson, M. C., Florence, Ala. (26).
Wilson, Prof. P. B., 44 Second St., Baltimore, Md. (30).
Wingate, Miss Hannah S., 208 Raymond St., Brooklyn, N. Y. (31).
Winslow, Sam'l W., Room 30, Rialto Building, 131 Devonshire, cor. Milk St., Boston, Mass. (29).
Winston, Prof. Chas. H., Richmond College, Richmond, Va. (30).

Witherbee, Frank S., care Witherbees, Sherman & Co., Port Henry, N. Y. (29).
Wolcott, Mrs. Henrietta L. T., Hotel Vendôme, Boston, Mass. (29).
WOOD, DR. ROBERT W., Jamaica Plain, Mass. (29).
Wood, Dr. Wm., Portland, Me. (29).
Woodbury, C. J. H., 131 Devonshire St., Boston, Mass. (29).
Woodward, J. J., M. D., Army Medical Museum, Washington. D. C. (28).
Woodward, Richard W., South Pueblo, Col. (29).
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Worthen, T. W. D., Hanover, N. H. (28).
WRIGHT, ELIZUR, Box 109, Boston, Mass. (31).
Wright, Harrison, Sec'y Wyoming Hist. and Geol. Soc., Wilkes Barré, Pa. (29).
Wright, Smithson E., Treasurer's Office, L. M. R. R. Co., Cincinnati, Ohio (30).
Wylie, Samuel B., Bloomington, Ind. (30).

Youmans, Wm. Jay, M. D., Popular Science Monthly, 549 Broadway, New York, N. Y. (28).
Young, Prof. Andrew H., Hanover, Ind. (30).

Zimmerman, Charles D., Buffalo, N. Y. (30).
Zimmerman, William, Room 22, 142 Dearborn St., Chicago, Ill. (30).
Zinke, E. Gustav, M. D., Elm St., near 14th, Cincinnati, Ohio (30).
Zwick, G. A., 11th and Madison Sts., Covington, Ky. (30).

[1407 MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

HONORARY FELLOW.[1]

*ROGERS, PROF. WILLIAM B., 117 Marlborough St., Boston, Mass. (1). 1881.

FELLOWS.[2]

Abbe, Prof. Cleveland, Washington, D. C. (16). 1874.
Agassiz, Alex., Curator Mus. Comp. Zool., Cambridge, Mass. (18). 1875.
Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20). 1874.
Allen, Dr. Harrison, 117 South 20th St., Philadelphia, Pa. (29). 1882.
Allen, Joel A., Mus. Comp. Zoology, Cambridge, Mass. (18). 1875.
Alvord, Benj., Brig. Gen., U. S. Army, 1207 Q St., N. W., Washington, D. C. (17). 1874.
Alvord, Henry E., C. E., Houghton Farm, Mountainville, Orange Co., N. Y. (29). 1882.
Anthony, Prof. Wm. A., Cornell Univ., Ithaca, N. Y. (28). 1880.
Appleton, Prof. John H., Brown Univ., Providence, R. I. (22). 1874.
Ashburner, Wm., 1014 Pine St., San Francisco, Cal. (29). 1882.
Atkinson, Edward, 131 Devonshire St., Boston, Mass. (29). 1881.
Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29). 1882.
Austen, Peter T., Ph. D., Rutgers College, Lock Box No. 2, New Brunswick, N. J. (26). 1879.
Austin, E. P., 46 E. Newton St., Boston, Mass. (18). 1874.

Bailey, Prof. Loring W., University of Fredericton, N. B. (18). 1875.
Bailey, Prof. W. W., Brown University, Providence, R. I. (18). 1874.
Baird, Prof. S. F., Sec'y Smithsonian Inst., Washington, D. C. (1). 1875.
Baker, Marcus, Coast Survey Office, Washington, D. C. (30). 1882.
Ballou, Geo. F., Waltham, Mass. (29). 1881.
Bandelier, Ad. F., Highland, Ill. (25). 1879.
BARKER, PROF. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1875.
Barnard, F. A. P., President Columbia College, New York, N. Y. (7). 1874.
Barnard, Prof. Wm. S., U. S. Dept. Agric., Washington, D. C. (24). 1880.
Bartlett, J. R., Comd'r U. S. N., care of C. & G. Survey Office, Washington, D. C. (30). 1882.
Bassett, Homer F., Waterbury, Conn. (23). 1874.
Batchelder, Dr. J. H., Salem, Mass. (18). 1874.
Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875.
Beal, Prof. Wm. James, Agricultural College, Lansing, Mich. (24). 1880.
Belknap, George E., Capt. U. S. N., care Fred. L. Wheeler & Co., Box 3522, Boston, Mass. (29). 1881.

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date at the end of the line the time when made a Fellow.

When the name is given in small capitals, it designates that the Fellow is also a life member, and is entitled to the Annual Volume of Proceedings.

* Deceased.

Bell, Dr. Alex. Graham, Scott Circle, 1500 Rhode Island Ave., Washington, D. C. (26). 1879.
Bell, Samuel N., Manchester, N. H. (7). 1874.
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21). 1880.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Bickmore, Prof. Albert S., American Museum of Natural History, Central Park, New York, N. Y. (17). 1880. [1882.
Blair, Henry W., U. S. C. & G. Survey Office, Washington, D. C. (26).
Blake, Clarence J., M. D., Hotel Berkeley, Boston, Mass. (24). 1877.
Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, Auburndale, Mass. (23). 1874.
Bolton, Dr. H. Carrington, Trinity Coll., Hartford, Conn. (17). 1875.
Borden, Spencer, Fall River, Mass. (29). 1882.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bouvé, Thos. T., Boston Soc. Nat. Hist., Boston, Mass. (1). 1875.
Bowditch, Prof. H. P., Harvard Med. School, Boston, Mass. (28). 1880.
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875.
Breneman, A. A., Ithaca, N. Y. (20). 1880.
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875.
Britton, N. L., Columbia College, New York, N. Y. (29). 1882.
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879.
Brooks, Major Thomas B., Geol. State Surveys, Mich. and Wis., Newburgh, N. Y. (26). 1879.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., care of Yale College Observatory, New Haven, Conn. (11). 1874.
Brown, Mrs. Robert, jr., New Haven, Conn. (17). 1874.
Brush, Prof. George J., Yale College, New Haven, Conn. (4). 1874.
Buckhout, W. A., State College, Centre Co., Pa. (20). 1881.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass. (22). 1877.
Burnham, S. W., 52 Vincennes Ave., Chicago, Ill. (25). 1877.
Burrill, Prof. T. J., Illinois Industrial Univ., Champaign, Ill. (29). 1882.

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875.
Call, Prof. R. Ellsworth, David City, Neb. (29). 1882.
Canby, Wm. M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878.
Capen, Rev. Francis L., Boston, Mass. (23). 1874.
Carhart, Prof. Henry S., Evanston, Ill. (29). 1881.
Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21). 1875.
Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
Carr, Lucien, Peabody Mus. Arch. and Ethn., Cambridge, Mass. (25). 1877.
Chamberlain, Wm. I., Sec. Ohio State Bd. of Agric., Columbus, Ohio (30). 1882.

Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., 50th St. cor. 4th Ave., New York, N. Y. (19). 1875. [1882.
Chandler, Seth C., jr., Harvard Observatory, Cambridge, Mass. (29).
Chandler, Prof. W. H., The Lehigh Univ., Bethlehem, Pa. (19). 1874.
Chanute, O., care Erie Railway, New York, N. Y. (17). 1877.
Chesbrough, E. S., Chicago, Ill. (2). 1877.
Chester, Prof. Albert H., Hamilton College, Clinton, N. Y. (29). 1882.
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877.
Chittenden, Russell H., Ph. D., 95 Humphrey St., New Haven, Conn. (29). 1882.
Clark, Alvan G., Cambridgeport, Mass. (28). 1880. [1875.
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17).
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18). 1874.
Claypole, Edw. W., B. A., New Bloomfield, Perry Co., Pa. (30). 1882.
Coffin, Prof. John H. C., U. S. Navy, Washington, D. C. (1). 1874.
Coffin, Prof. Selden J., Ph.D., Lafayette College, Easton, Pa. (22). 1874.
Collett, Prof. John, Chief Indiana Bureau of Statistics of Geology, Indianapolis, Ind. (17). 1874.
Collier, Peter, Dept. of Agric., Washington, D. C. (29). 1882.
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880.
Comstock, J. Henry, Cornell Univ., Ithaca, N. Y. (28). 1882.
Comstock, Prof. M. L., Ph. D., Galesburg, Knox Co., Ill. (21). 1874.
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24). 1880.
Cook, Prof. George H., New Brunswick, N. J. (4). 1875.
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880.
Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1875.
Cox, Prof. Edward T. (19). 1874.
Cox, Hon. Jacob D., 103 Broadway, Cincinnati, Ohio (30). 1881.
Coxe, Eckley B., Drifton, Luzerne Co., Pa. (23). 1879.
Crocker, Susan E., M.D., Lawrence, Mass. (21). 1874.
Crosby, Wm. O., Mass. Inst. Technology, Boston, Mass. (29). 1881.
Cross, Prof. Chas. R., Mass. Inst. Technology, Boston, Mass. (29). 1880.
Cummings, Rev. Joseph, D.D., Pres. Northwestern Univ., Evanston, Ill. (13). 1874.
Curtis, Dr. Josiah, Washington, D. C. (18). 1874.
Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17). 1874.

Dabney, Chas. W., jr., Ph. D., Agric. Experiment Station, Raleigh, N. C. (30). 1882.
Dall, Mrs. Caroline H., 31st St., Washington, D. C. (18). 1874.
Dall, Wm. H., Smithsonian Institution, Washington, D. C. (18). 1874.
Dana, Edward Salisbury, New Haven, Conn. (23). 1875.
Dana, Prof. James D., New Haven, Conn. (1). 1875.

Danforth, Edward, Dep't of Public Instruction, Elmira, N. Y. (11). 1874.
Davidson, Prof. Geo., Asst. Coast and Geodetic Survey, San Francisco, Cal. (29). 1881.
Dawson, Dr. J. W., Principal McGill Coll., Montreal, Canada (10). 1875.
Day, F. H., M. D., Wauwatosa, Wis. (20). 1874.
Dean, George W., U. S. Coast Survey, Fall River, Mass. (15). 1874.
Dimmock, George, Cambridge, Mass. (22). 1874.
Dinwiddie, Robert, 117 W. 43d St., New York, N. Y. (1). 1874.
Dodge, Charles R., Washington, D. C. (22). 1874.
Dolbear, A. Emerson, College Hill, Mass. (20). 1880.
Draper, Dan'l, Ph.D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Avenue, New York, N. Y. (29). 1881.
Drown, Prof. Thos. M., Lafayette College, Easton, Pa. (29). 1881.
Du Bois, Prof. Aug. J., New Haven, Conn. (30). 1882.
Dudley, Charles B., Altoona, Pa. (23). 1882.
Dudley, Wm. L., Miami Med. College, Cincinnati, Ohio (28). 1881.
Dunnington, Prof. F. P., University of Virginia, Va. (26). 1880.
Dutton, Capt. C. E., U. S. Ord. Dep't, Washington, D. C. (23). 1875.
Dwight, Prof. Wm. B., Vassar College, Poughkeepsie, N. Y. (30). 1882.
Dwyer, John, M. D., Hartford, Conn. (23). 1874.

Eads, Jas. B., 314 Chestnut Street, St. Louis, Mo. (27). 1879.
Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). 1879.
Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19). 1874.
Eddy, Prof. H. T., University of Cincinnati, Cincinnati, Ohio (24). 1875.
Edison, Thos. A., Menlo Park, N. J. (27). 1878.
Edmands, J. Rayner, Observatory, Cambridge, Mass. (29). 1880.
Egleston, Prof. Thos., School of Mines, Columbia College, New York, N. Y. (27). 1879.
Eimbeck, Wm., U.S.C.S., Washington, D. C. (17). 1874.
Elliott, Arthur H., School of Mines, Columbia College, New York, N. Y. (23). 1880.
Elliott, Ezekiel B., Treasury Dept., Washington, D. C. (10). 1874.
Elsberg, Louis, M.D., 614 Fifth Avenue, New York, N. Y. (23). 1874.
Emerson, Prof. Benjamin K., Amherst, Mass. (19). 1877.
Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22). 1874.
Emerton, James H., Salem, Mass. (18). 1875.
Emmons, S. F., Box 2836, Denver, Col. (26). 1879.
Engelmann, Dr. George, St. Louis, Mo. (1). 1875.
Engelmann, Geo. J., M. D., 3003 Locust St., St. Louis, Mo. (25). 1878.
Ernst, Carl W., Daily Advertiser, Boston, Mass. (23). 1874.
Eustis, Prof. Henry L., Cambridge, Mass. (2). 1874.
Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Henry, Ph. D., St. Johnsbury, Vt. (14). 1874.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1875.

Farmer, Moses G., Torpedo Station, Newport, R. I. (9). 1875.
Farquharson, Dr. Robert James, Des Moines, Iowa (24). 1880.
Ferguson, Maj. Thos. B., Ass't U. S. Fish Commissioner, Washington, D. C. (28). 1881.
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (22). 1881.
Ferrel, Wm., U. S. Coast Survey Office, Washington, D. C. (11). 1875.
Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874.
Fletcher, Dr. Robert, Surgeon General's Office, Washington, D. C. (29). 1881.
Flint, James M., Surgeon U. S. N., 42 Dartmouth St., Boston, Mass. (28). 1882.
Foote, Dr A. E., 1223 Belmont Avenue, Philadelphia, Pa. (21). 1874.
Forbes, Prof. S. A., Normal, Ill. (27). 1879.
Frazer, Dr. Persifor, Ass't Geologist, 2d Geol. Survey of Pa., 917 Clinton St., Philadelphia, Pa. (24). 1879.
Frazier, Prof. B. W., The Lehigh University, Bethlehem, Pa. (24). 1882.
Freeman, H. C., Alto Pass, Union Co., Ill. (17). 1876.
Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880.
Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882.

Gage, Simon Henry, Ithaca, N. Y. (28). 1881.
Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23). 1874.
Gardner, James T., Director N. Y. State Survey, Albany, N. Y. (25). 1879.
Garland, Rev. Dr. L. C., Chancellor Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Garman, S., Museum Comp. Zoology, Cambridge, Mass. (20). 1874.
Gatschet, Albert S., Bureau of Ethnology, Washington, D. C. (30). 1882.
Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1875.
Gilbert, G. K., Box 240, Salt Lake City, Utah (18). 1874.
Gillman, Henry, 155 Adams Avenue East, Detroit, Mich. (24). 1875.
Gilman, Daniel C., President of the Johns Hopkins University, Baltimore, Md. (10). 1875.
Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1875.
Goldschmidt, S. A., Ph. D., Morse Building, 140 Nassau St., New York, N. Y. (24). 1880.
Gooch, Frank A., U. S. Geological Survey, Newport, R. I. (25). 1880.
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875.
Goode, Prof. G. Brown, Curator National Museum, Washington, D. C. (22). 1874.
Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1879.
Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874.
Gray, Prof. Asa, Botanic Gardens, Cambridge, Mass. (1). 1875.
Green, Traill, M. D., Easton, Pa. (1). 1874.
Grimes, J. Stanley (17). 1874.

Grote, Aug. R., New Brighton, Staten Island, N. Y. (22). 1875.
Guyot, Prof. Arnold, Princeton, N. J. (1). 1875.

Hagen, Dr. Hermann A., Mus. Comp. Zool., Cambridge, Mass. (17). 1875.
Hague, Arnold, 10 West 29th St., New York (26). 1879.
Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877.
Hall, Edwin H., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Hall, Prof. James, Albany, N. Y. (1). 1875.
Hale, Horatio, Clinton, Ontario, Can. (30). 1882.
Hamlin, Dr. A. C., Bangor, Me. (10). 1874.
Harger, Oscar, Yale College Museum, New Haven, Conn. (25). 1879.
Harkness, Prof. Wm., U. S. N., U. S. Observatory, Washington, D. C. (26). 1878.
Harrington, Prof. Mark W., Ann Arbor, Mich. (22). 1875.
Harrison, Dr. B. F., Wallingford, Conn. (11). 1874.
Hasbrouck, Prof. I. E., Rutgers Coll., New Brunswick, N. J. (23). 1874.
Haskins, Rev. David Greene, Cambridge, Mass. (25). 1874.
Hastings, C. S., Johns Hopkins University, Baltimore, Md. (25). 1878.
Hazen, Brig. Gen. Wm. B., Chief Signal Office, U. S. A., Washington, D. C. (30). 1882.
Hedrick, B. S., Exam'r U. S. Pat. Office, Washington, D. C. (19). 1874.
Henderson, John G., Winchester, Ill. (25). 1879.
Henshaw, Henry W., Care Major J. W. Powell, 910 M St., Washington, D. C. (24). 1877.
Hervey, Rev. A. B., Taunton, Mass. (22). 1879.
Hilgard, Prof. E. W., Univ. of California, Berkeley, Cal. (11). 1874.
Hilgard, Prof. J. E., U. S. Coast Survey, Washington, D. C. (4). 1874.
Hill, Franklin C., P. O. Box 338, Princeton, N. J. (29). 1881.
Hill, George W., Nautical Almanac Office, Navy Department, Washington, D. C. (23). 1874.
Hill, Rev. Dr. Thomas, 738 Congress St., Portland, Me. (3). 1875.
Hill, Walter N., Chester, Pa. (29). 1881.
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882.
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874.
Hitchcock, Romyn, 53 Maiden Lane, New York, N. Y. (29). 1881.
Hodges, N. D. C., Salem, Mass. (29). 1882.
Hoffmann, Dr. Fred., 797 Sixth Ave., New York, N. Y. (28). 1881.
Holden, Prof. E. S., Director Washburn Observatory, Univ. of Wisconsin, Madison, Wis. (23). 1875.
Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881.
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876.
Hough, Franklin B., Lowville, N. Y. (4). 1874.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Montreal, Canada (1). 1874.
Huntington, Prof. J. H., Boston, Mass. (19). 1874.
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875.

Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10). 1874.
Hyde, Prof. E. W., Walnut Hills, Cincinnati, Ohio (25). 1881.

James, Jos. F., 177 Race St., Cincinnati, Ohio (30). 1882.
Jeffries, B. Joy, M. D., 15 Chestnut St., Boston, Mass. (29). 1881.
Jenks, Elisha T., Middleborough, Mass. (22). 1874.
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874.
Jewell, Theo. F., Lt. Comdr. U.S.N., Torpedo Station, Newport, R. I. (25). 1882.
Jillson, Dr. B. C., Illinois Industrial Univ., Champaign, Ill. (14). 1881.
Johnson, Prof. S. W., Sheffield Sci. School, New Haven, Conn. (22). 1874.
Johnson, Prof. W. W., Annapolis, Md. (29). 1881.
Joy, Prof. Charles A., Stockbridge, Mass. (8). 1879.
Julien, A. A., School of Mines, Columbia Coll., New York, N. Y. (24). 1875.

Kedzie, Prof. Robert C., Lansing, Mich. (29). 1881.
Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882.
Kennan, George, 1318 Mass. Avenue, Washington, D. C. (30). 1882.
Kent, William, Pittsburgh, Pa. (26). 1881.
Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 1882.
Langley, Prof. J. W., Univ. of Michigan, Ann Arbor, Mich. (23). 1875.
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18). 1874.
Lanza, Prof. Gaetano, Mass. Inst. of Technology, Boston, Mass. (29). 1882.
Lattimore, Prof. S. A., Univ. of Rochester, Rochester, N. Y. (15). 1874.
Lawrence, George N., 45 E 21st St., New York, N. Y. (7). 1877.
Lazenby, Prof. Wm. R., Ithaca, N. Y. (30). 1882.
Lea, Dr. Isaac, 1622 Locust St., Philadelphia, Pa. (1). 1875.
LeConte, Dr. John L., 1625 Spruce St., Philadelphia, Pa. (1). 1874.
LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881.
Ledoux, Albert R., Ph. D., 17 Cedar St., New York, N. Y. (26). 1881.
Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874.
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Philadelphia, Pa. (2). 1874.
Lewis, Prof. Henry C., E. Washington Lane, Germantown, Pa. (26). 1880.
Lilly, Gen. Wm., Mauch Chunk, Carbon Co., Pa. (28). 1882.
Lindsley, Dr. J. Berrien, Nashville, Tenn. (1). 1874.
Lintner, J. A., N. Y. State Mus. of Nat. Hist., Albany, N. Y. (22). 1874.
Little, W. C., Albany, N. Y. (22). 1874.
Litton, Abram, Washington University, St. Louis, Mo. (28). 1879.
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 1875.
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.

Lord, Prof. Nat. W., State Univ., Columbus, Ohio (29). 1881.
Loudon, Prof. James, Toronto, Can. (25). 1881.
Loughridge, Prof. R. H., Berkeley, Cal. (21). 1874.
Love, Edward G., School of Mines, Columbia College, New York, N. Y. (24). 1882.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1875.
Lull, Edward P., Captain U. S. N., Navy Yard, Boston, Mass. (28). 1880.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyle, David Alexander, Capt. of Ordnance U. S. A., National Armory, Springfield, Mass. (28). 1880.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1875.
Lyman, Hon. Theodore, Brookline, Mass. (23). 1875.
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

Mabery, C. F., Cambridge, Mass. (29). 1881.
Macfarlane, James, Towanda, Pa. (29). 1882.
Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882.
Mallery, Brevet Lieut. Col. Garrick, U. S. Army, Box 585, Washington, D. C. (26). 1879.
Mann, B. Pickman, U. S. Dep't of Agric., Washington, D. C. (22). 1874.
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874.
Mark, Edw. L., 48 Shepard St., Cambridge, Mass. (21). 1879.
Markoe, Geo. F. H., 61 Warren St., Roxbury, Mass. (29). 1881.
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15). 1874.
Martin, Prof. Daniel S., Rutgers Female College, New York, N. Y. (23). 1879.
Martin, Prof. H. Newell, Johns Hopkins Univ., Baltimore, Md. (27). 1880.
Mason, Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayer, Prof. A. M., So. Orange, N. J. (19). 1874.
McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., 321 Michigan Ave., Chicago, Ill. (29). 1881.
McGee, W. J., U. S. Geol. Survey, Salt Lake City, Utah (27). 1882.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McNair, Lt. Commander Antoine R., U. S. N., Box 1071, Saratoga Springs, N. Y. (28). 1882.
McRae, Hamilton S., Supt. of Schools, Muncie, Ind. (20). 1874.
Meehan, Thomas, Germantown, Pa. (17). 1875.
Mees, Carl Leo, Louisville, Ky. (24). 1876.
Mendenhall, Prof. T. C., Columbus, Ohio (20). 1874.
Merriman, C. C., Rochester, N. Y. (29). 1880.
Michelson, A. A., Master U. S. N., Nautical Almanac Office, Washington, D. C. (26). 1879.
Minot, Charles Sedgwick, Roslindale P. O., Boston, Mass. (28). 1880.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882.

Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22). 1874.
Morley, Prof. Edward W., Hudson, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875.
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22). 1874.
Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13). 1874.
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882.
Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24). 1877.
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia College, New York, N. Y. (5). 1875.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13). 1874.
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, E. L., Ph. D., Central Univ. Richmond, Ky. (28). 1881.
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18). 1875.
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876.
Norton, Prof. W. A., New Haven, Conn. (6). 1875.
Norwood, Charles J., Ass't Kentucky Geological Survey, Russellville, Ky. (26). 1881.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1875.
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 1874.
Ordway, Prof. John M., Mass. Inst. Technology, Boston, Mass. (9). 1875.
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875.
Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22). 1874.
Osler, Prof. William, McGill College, Montreal, Canada (28). 1881.
Owen, Dr. Richard, New Harmony, Ind. (20). 1874.

Packard, Dr. A. S., jr., 115 Angell St., Providence, R. I. (16). 1875.
Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parke, John G., Lt. Col. Corps of Eng'rs, Bv't Maj. Gen. U. S. A., Office of Chief of Engineers, Washington, D. C. (29). 1881.
Parkhurst, H. M., Law Stenographer, 25 Chambers St., New York, N. Y. (23). 1874.
Peckham, S. F., Providence, R. I. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1875.
Peet, Rev. Stephen D., Clinton, Wis. (24). 1881.
Peirce, B. O., Beverly, Mass. (18). 1874.
Penhallow, Prof. D. P., Experiment Station, Mountainville, N. Y. (30). 1882.

Perkins, Prof. George H., Burlington, Vt. (17). 1882.
Perkins, Maurice, Schenectady, N. Y. (15). 1875.
Peter, Dr. Robert, Ky. Geol. Survey, Lexington, Ky. (29). 1881.
Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.
Phillips, A. W., New Haven, Conn. (24). 1879.
Phippen, Geo. D., Salem, Mass. (18). 1874.
Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1875.
Pike, Dr. William H., University College, Toronto, Can. (29). 1881.
Pilling, James C., Box 585, Washington, D. C. (28). 1882.
Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 615 Walnut St., Philadelphia, Pa. (27). 1882.
Potter, Prof. William B., Washington University, St. Louis, Mo. (25). 1879.
Powell, Major J. W., U. S. Geologist, 910 M St., Washington, D. C. (23). 1875.
Prentiss, D. Webster, M. D., Washington, D. C. (29). 1882.
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875.
Prime, Frederick, jr., Allentown Iron Co., 230 So. 3d St., Philadelphia, Pa. (24). 1875.
Pritchett, Henry S., Observatory, Washington Univ., St. Louis, Mo. (29). 1881.
Procter, John R., Director Ky. Geol. Survey, Frankfort, Ky. (26). 1881.
Pulsifer, Wm. H., St. Louis, Mo. (26). 1879.
Pumpelly, Prof. Raphael, U. S. Geol. Survey, Newport, R. I. (17). 1875.
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.
Putnam, James J., M. D., 63 Marlborough St., Boston, Mass. (28). 1880.
Pynchon, Rev. T. R., Pres. Trinity College, Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.
Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.
Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875.
Redfield, J. H., care A. Whitney & Sons, Philadelphia, Pa. (1). 1874.
Reed, E. Baynes, London, Ontario, Can. (27). 1882.
Rees, Prof. John K., Washington University, St. Louis, Mo. (26). 1878.
Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875.
Rice, John M., U. S. Naval Academy, Annapolis, Md. (25). 1881.
Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.
Richards, Mrs. Robert H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.
Ricketts, Pierre de Peyster, Ph.D., 10 Cedar St., New York, N. Y. (26). 1880.

RILEY, PROF. C. V., U. S. Entomologist, 1700 13th St. N.W., Washington, D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Rockwell, Gen. Alfred P., Office Great Falls Manufacturing Co., Boston, Mass. (10). 1882.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, Pa. (11). 1874.
Rogers, Prof. Robert E., Jefferson Medical College, Philadelphia, Pa. (18). 1874.
Rogers, W. A., Ass't Prof. Harvard College Observ., Cambridge, Mass. (15). 1875.
Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879.
Rood, Prof. O. N., Cor. E. 49th St. & Madison Ave., New York, N. Y. (14). 1875.
Roosevelt, Clinton, 11 Wall St., New York, N. Y. (11). 1874.
Ross, Waldo O., 189 Devonshire St., Boston, Mass. (29). 1882.
Rowland, Prof. Henry A., Baltimore, Md. (29). 1880.
Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1875.
Russell, I. C., U. S. Geological Survey, Salt Lake City, Utah (25). 1882.
Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (13). 1875.

Safford, Dr. James M., Nashville, Tenn. (6). 1875.
Sampson, Comdr. W. T., U. S. N., Naval Observatory, Washington, D. C. (25). 1881.
Saunders, William, London, Ontario, Canada (17). 1874.
Schanck, Prof. J. Stillwell, Princeton, N. J. (4). 1882.
Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874.
Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24). 1877.
SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874.
Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, D. C. (23). 1874.
Seiler, Carl, M. D., Philadelphia, Pa. (29). 1882.
Sestini, Rev. B., Woodstock, Md. (29). 1882.
Sheafer, P. W., Pottsville, Pa. (4). 1879.
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.
Sigsbee, Chas. D., Comd'r U. S. N., Navy Dep't, Washington, D. C. (28). 1882.
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.
Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1874.
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19). 1874.
Skinner, Joseph J., New Britain, Conn. (23). 1880.
Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20). 1877.
Smith, Mrs. Erminnie A., 203 Pacific Ave., Jersey City, N. J. (25). 1880.
Smith, Prof. F. H., University of Virginia, Charlottesville, Va. (26). 1880.
Smith, Dr. J. Lawrence, Louisville, Ky. (1). 1874.
SMITH, QUINTIUS C., M. D., Box 707, Austin, Texas (26). 1881.

Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1875.
Smock, Prof. John Conover, Box 282, New Brunswick, N. J. (23). 1879.
Snow, Prof. F. H., Lawrence, Kan. (29). 1881.
Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882.
Spencer, Prof. J. William, Univ. of Missouri, Columbia, Mo. (28). 1882.
Spencer, John W., Geol., Paxton, Sullivan Co., Ind. (20). 1874.
Springer, Dr. Alfred, Box 573, Cincinnati, Ohio (24). 1880.
Stallo, J. B., Masonic Temple, Cincinnati, Ohio (30). 1882.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
Stephens, W. Hudson, Lowville, N. Y. (18). 1874.
Sternberg, George M., Surgeon U.S.A., Care Surgeon General, U.S.A., Washington, D. C. (24). 1880.
Stevens, W. LeConte, 170 Joralemon St., Brooklyn, N. Y. (29). 1882.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1875.
Stone, George H., Colorado Springs, Col. (29). 1882.
Stone, Mrs. Leander, 3352 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Leander McCormick Observatory, Univ. of Va., Va. (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Story, Wm. E., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874.
Sturtevant, E. Lewis, M. D., Geneva, N. Y. (29). 1882.
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20). 1881.
Swallow, Prof. G. C., Columbia, Mo. (10). 1875.
Swift, Lewis, Ph. D., Rochester, N. Y. (29). 1882.

Tainter, Sumner, 1221 Connecticut Ave., Washington, D. C. (29). 1881.
Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Taylor, Arthur F., Ph. D., Case School of Applied Science, Cleveland, Ohio (29). 1882.
Taylor, Wm. B., Smithsonian Institution, Washington, D. C. (29). 1881.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thomas, Benj. F., Ph. D., Univ. State of Missouri, Columbia, Mo. (29). 1882.
Thurston, Prof. R. H., Stevens Institute, Hoboken, N. J. (23). 1875.
Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881.
Townshend, Prof. N. S., Columbus, Ohio (17). 1881.
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27). 1881.
Trembley, J. B., M.D., Oakland, Alameda Co., Cal. (17). 1880.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25). 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.

True, Fred W., U. S. National Museum, Washington, D. C. (28). 1882.
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.
Upham, Warren, Nashua, N. H. (25). 1880.

Van der Weyde, P. H., M. D., Box 3619, New York, N. Y. (17). 1874.
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882.
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16). 1875.
Vose, Prof. George L., Mass. Inst. Technology, Boston, Mass. (15). 1875.

Wadsworth, M. Edward, Ph. D., Assistant in Lithology, Museum of Comparative Zoology, Cambridge, Mass. (23). 1874.
Walcott, Charles D., U. S. Geol. Survey, Washington, D. C. (25). 1882.
Waldo, Leonard, S. D., New Haven, Conn. (28). 1880.
Walker, Prof. J. R., 154 Canal St., New Orleans, La. (19). 1874.
Walker, Prof. Joseph B. (20). 1874.
Wallace, Wm., Ansonia, Conn. (28). 1882.
Waller, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walling, H. F., U. S. Coast and Geodetic Survey Office, Washington, D. C. (16). 1874.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875.
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
Warder, Prof. Robert B., North Bend, Hamilton Co., Ohio (19). 1881.
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18). 1874.
Warren, Cyrus M., Brookline, Mass. (29). 1882.
Warren, Prof. S. Edward, Newton, Mass. (17). 1875.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1875.
Wead, Prof. Charles K., Univ. of Michigan, Ann Arbor, Mich. (23). 1880.
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23). 1882.
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (7). 1874.
Wells, Daniel H., Hartford, Conn. (18). 1875.
Westcott, O. S., Maywood, Ill. (21). 1874.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheeler, Capt. Geo. M., U. S. E., Lock Box 93, Washington, D. C. (25). 1879.
Wheeler, Orlando B., Ass't U. S. Lake Survey, Detroit, Mich. (24). 1882.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875.
White, Prof. I. C., Univ. of W. Va., Morgantown, W. Va. (25). 1882.

Whitfield, R. P., American Museum Natural History, 77th St. & 8th Avenue, New York, N. Y. (18). 1874.
Whitman, Chas. O., care G. F. Leonard, Newton Highlands, Mass. (27). 1889.
Whitney, Solon F., Watertown, Mass. (20). 1874.
Whittlesey, Col. Charles, 1305 Euclid Ave., Cleveland, Ohio (1). 1875.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875.
Wiley, Prof. Harvey W., Dept. of Agric., Washington, D. C. (21). 1874.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, H. S., Cornell Univ., Ithaca, N. Y. (18). 1882.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, President University College, 117 Bloor St., Toronto, Canada (25). 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1875.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Winsor, Justin, Librarian Harvard Univ., Cambridge, Mass. (29). 1882.
Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29). 1881.
Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875.
Wyckoff, Wm. C., 44 Howard St., New York, N. Y. (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Youmans, Prof. Edward L., New York, N. Y. (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874.

Zentmayer, Joseph, 147 So. Fourth St., Philadelphia, Pa. (29). 1882.

[515 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1922.

DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.

Abert, J. J., Washington, D. C. (1). Born in 1785. Died January 27, 1863.

Adams, C. B., Amherst, Mass. (1). Born January 11, 1814. Died Jan'y [19, 1853.

Adams, Edwin F., Charlestown, Mass. (18).

Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.

Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec. [14, 1873.

Ainsworth, J. G., Barry, Mass. (14).

Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.

Allen, Zachariah, Providence, R. I. (1). Died March 17, 1882.

Allston, R. F. W., Georgetown, S. C. (3). Born April 21, 1801. Died April 7, 1864.

Ames, M. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.

Andrews, Ebenezer B., Lancaster, Ohio (7). Died Aug. 21, 1880, aged 59.

Anthony, Charles H., Albany, N. Y. (6). Died in 1874.

Appleton, Nathan, Boston, Mass. (1). Born Oct. 6, 1779. Died July 14, 1861.

Armstrong, John W., Fredonia, N. Y. (24).

Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11, 1878.

Aufrecht, Louis, Cincinnati, Ohio (30).

Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.

Bache, Franklin, Philadelphia, Pa. (1). Born Oct. 25, 1792. Died March 19, 1864.

Bailey, Jacob W., West Point, N. Y. (1). Born April 29, 1811. Died Feb. 26, 1857.

Bardwell, F. W., Lawrence, Kan. (13). Died in 1878.

Barnard, John G., New York, N. Y. (14). Died May 14, 1882.

Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.

Barry, Redmond, Melbourne, Australia (25).

Beck, C. F., Philadelphia, Pa. (1).

Beck, Lewis C., New Brunswick, N. J. (1). Born Oct. 4, 1798. Died April 21, 1853.

Beck, T. Romeyn, Albany, N. Y. (1). Born Aug. 11, 1791. Died Nov. 19, 1855.

Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.
Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.
Benedict, George W., Burlington, Vt. (16). Born Jan. 11, 1796. Died in 1871.
Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19,1877.
Binney, Amos, Boston, Mass. (1). Born Oct. 18, 1803. Died Feb. 18, [1847.
Binney, John, Boston, Mass. (3).
Blackie, Geo. S., Nashville, Tenn. (26).
Blake, Homer C., New York, N. Y. (28). Born Feb. 1, 1822.
Blanding, William, ———, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.
Bomford, George, Washington, D. C. (1). Born 1780. Died March 25, 1848.
Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.
Briggs, Robert, Philadelphia, Pa. (29).
Brigham, Charles H., Ann Arbor, Mich. (17). Born July 27, 1820. Died in Jan., 1879.
Brown, Andrew, Natchez, Miss. (1).
Burnap, G. W., Baltimore, Md. (12). Born Nov. 30, 1802. Died Sept. 8, 1859.
Burnett, Waldo I., Boston, Mass. (1). Died July 1, 1854, aged 27.
Butler, Thomas B., Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, F. A., New York, N. Y. (27). Died in 1879.
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5, 1880.
Case, William, Cleveland, Ohio (6).
Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died Jan. 8, 1877.
Chadbourne, Paul Ansel, Amherst, Mass. (10). Born Oct. 21, 1823. Died Feb. 23, 1883.
Chapman, N., Philadelphia, Pa. (1). Born May 28, 1780. Died July 1, 1853.
Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.
Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec. 13, 1870.
Cheney, Miss Margaret S., Jamaica Plain, Mass. (29).
Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.

Clark, Henry James, Cambridge, Mass. (13). Died July 1, 1873, aged 47.
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1). Born Sept. 6, 1806. Died Feb. 6, 1873.
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.
Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6). Born Dec. 14, 1794. Died April 9, 1872.
Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.
Cramp, J. M., Wolfville, N. S. (11). Born July 25, 1796. Died Dec. 6, 1881.
Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 13, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.
Croswell, Edwin, Albany, N. Y. (6). Born in May, 1797. Died June 13,
Curry, W. F., Geneva, N. Y. (11). [1871.

Dalrymple, E. A., Baltimore, Md. (11). Died Oct. 30, 1881.
Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.
Dean, Amos, Albany, N. Y. (6). Born Jan. 16, 1803. Died Jan. 26, 1868.
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York, N. Y. (1). Born in 1792. Died Nov. 21, 1851.
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1). Born Oct. 25, 1781. Died Dec. 15,
Dexter, G. M., Boston, Mass. (11). [1867.
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Dorr, E. P., Buffalo, N. Y. (25). Died March 28, 1881.
Draper, Henry, New York, N. Y. (28).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Born July 4, 1794. Died June 26, 1869.
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10). Died Aug. 9, 1855, aged 54.
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14).
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emerson, Geo. B., Boston, Mass. (1). Born Sept. 12, 1797. Died March 4, 1881.
Emmons, Ebenezer, Williamstown, Mass. (1). Born May 16, 1799. Died October 1, 1863.
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2). Born April 11, 1794. Died Jan. 15, 1865.
Ewing, Thomas, Lancaster, Ohio (5). Born Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York, N. Y. (6). Born Oct. 9, 1798. Died June 16, 1873.
Feuchtwanger, Lewis, New York, N. Y. (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7). Born Jan. 7, 1800. Died March 8, [1874.
Fisher, Mark, Trenton, N. J. (10).
Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.
Fitch, O. H., Ashtabula, Ohio (7). Died Sept. 17, 1882, in his 80th year.
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born Nov. 26, 1790. Died Jan. 23, 1868.
Ford, A. C., Nashville, Tenn. (26).
Forshey, Caleb G., New Orleans, La. (21). Died in Aug., 1881.
Foster, John W., Chicago, Ill. (1). Born March 4, 1815. Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Born Oct. 17, 1795. Died Feb. 6, [1865.
Fox, Charles, Grosse Ile, Mich. (7).
Frazer, John F., Philadelphia, Pa. (1). Born July 8, 1812. Died Oct. 14, [1872.
French, J. W., West Point, N. Y. (11).

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.
Gavit, John E., New York, N. Y. (1).
Gay, Martin, Boston, Mass. (1). Died Jan. 12, 1850, aged 46.
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10). Born in 1816. Died Jan'y 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gould, Augustus A., Boston, Mass. (11). Born April 28, 1805. Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2). Born June 15, 1787. Died Oct. 24, 1859.

Graham, James D., Washington, D. C. (1). Born in 1799. Died Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in 1808. Died March 10, 1860.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19). Born in 1822. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York, N. Y. (4). Born March 9, 1809. Died January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, S. S., Chickies, Pa. (1). Died Sept. 10, 1880, aged 68.
Hale, Enoch, Boston, Mass. (1). Born Jan. 29, 1790. Died Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (1). Born Jan. 17, 1781. Died May 15, 1858.
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, A. M., Plymouth, Mass. (29).
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1). Died April 20, 1853, aged 57.
Hartt, Charles F., Ithaca, N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Born Jan. 4, 1816. Died May 23, 1874.
Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1). Died July 27, 1866, aged 80.
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878.
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.
Hilgard, Theo. C., St. Louis, Mo. (17). Born Feb. 28, 1828. Died Mch. 5, 1875.
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1). Born May 24, 1793. Died Feb. 27, 1864.
Hodgson, W. B., Savannah, Ga. (10). Born 1815.
Holbrook, John E., Charleston, S. C. (1). Born Dec. 31, 1796. Died Sept. 8, 1871.
Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.
Hopkins, Wm., Lima, N. Y. (5.) Died in March, 1867.

Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Houghton, Douglas, Detroit, Mich. (1). Born Sept. 21, 1809. Died Oct. 13, 1845.
Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, Edward B., Washington, D. C. (2). Born June 15, 1822. Died Oct. 2, 1863.
Hunt, Freeman, New York, N. Y. (11). Born March 21, 1804. Died March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles T., Boston, Mass. (1). Born June 21, 1805. Died Aug. 29, 1880.
James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.
Johnson, Walter R., Washington, D. C. (1). Born June 21, 1794. Died April 26, 1852.
Jones, Catesby A. R., Washington, D. C. (8).
Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).
Keely, George W., Waterville, Me. (1). Died in 1878.
Keep, N. C., Boston, Mass. (13). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.
King, Mitchell, Charleston, S. C. (3). Born June 8, 1788. Died in 1862.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died
LaRoche, R., Philadelphia, Pa. (12). [Sept. 14, 1875.
Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died in 1852.
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (3). Born Sept. 3, 1830. Died June
Lincklaen, Ledyard, Cazenovia, N. Y. (1). [27, 1862.
Linsley, James H., Stafford, Conn. (1). Born May 5, 1787. Died Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.

Logan, William E., Montreal, Canada (1). Born April 23, 1798. Died June 22, 1875.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June 24, 1872.

Maack, G. A., Cambridge, Mass. (18).
Mahan, Dennis H., West Point, N. Y. (9). Born April 2, 1802. Died Sept. 16, 1871.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10).
Mather, William W., Columbus, Ohio (1). Born May 24, 1804. Died Feb. 27, 1859.
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
McMahon, Mathew, Albany, N. Y. (11).
M'Conihe, Isaac, Troy, N. Y. (5).
Meade, George G., Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.
Minifie, William, Baltimore, Md. (12). Born in 1805. Died Oct. 24, 1880.
Mitchel, O. M., Cincinnati, Ohio (3). Born Aug. 28, 1810. Died Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (2). Died April 2, 1869, aged 76.
Mitchell, Wm. H., Florence, Ala. (17).
Morgan, Lewis H., Rochester, N. Y. (10). Born Nov. 21, 1818. Died Dec. 17, 1881.
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel G., Philadelphia, Pa. (1). Born Jan. 26, 1799. Died May 15, 1851.
Mudge, Benjamin F., Manhattan, Kansas (25). Born Aug. 11, 1817. Died Nov. 21, 1879, aged 62.
Munroe, Nathan, Bradford, Mass. (6). Born May 16, 1804. Died July 8, [1866.
Munroe, William, Concord, Mass. (18). Died April 27, 1877.
Mussey, William H., Cincinnati, Ohio (30). Born Sept. 13, 1818. Died Aug. 1, 1882.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.
Nicholson, Thomas, New Orleans, La. (21).

Nicollett, Jean N., Washington, D. C. (1). Born July 24, 1786. Died Sept. 11, 1843.

Norton, John P., New Haven, Conn. (1). Born in July, 1822. Died Sept. 5, 1852.

Noyes, J. O., New Orleans, La. (21).

Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.

Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.

Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.

Olmsted, Alexander F., New Haven, Conn. (4). Born Dec. 20, 1822. Died May 5, 1853.

Olmsted, Denison, New Haven, Conn. (1). Born June 18, 1791. Died May 13, 1859.

Olmsted, Denison, jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.

Otis, George Alexander, Washington, D. C. (10). Born Nov. 12, 1830. Died Feb. 23, 1881.

Painter, Jacob, Lima, Pa. (23). Died in 1876.

Painter, Minshall, Lima, Pa. (7).

Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.

Parkman, Samuel, Boston, Mass. (1). Died Dec. 15, 1854, aged 38.

Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.

Peirce, Benjamin, Cambridge, Mass. (1). Born April 4, 1809. Died Oct. 6, 1880.

Perkins, George R., Utica, N. Y. (1). Born May 3, 1812. Died Aug. 22, 1876.

Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 18, 1804. Died Feb. 2, 1873.

Perry, John B., Cambridge, Mass. (16). Died Oct. 3, 1872, aged 52.

Perry, M. C., New York, N. Y. (10).

Piggot, A. Snowden, Baltimore, Md. (10).

Plumb, Ovid, Salisbury, Conn. (9).

Pope, Charles A., St. Louis, Mo. (12). Born May 15, 1818. Died July 6, 1870.

Porter, John A., New Haven, Conn. (14). Born March 15, 1822. Died Aug. 25, 1866.

Pourtalès, Louis François de, Cambridge, Mass. (1). Born in 1822. Died July 18, 1880.

Pruyn, John V. L., Albany, N. Y. (1). Born June 22, 1811. Died Nov. 21, 1877.

Pugh, Evan, Centre Co., Pa. (14).

Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.

Putnam, J. Duncan, Davenport, Iowa. (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.
Redfield, William C., New York, N. Y. (1). Born March 26, 1789. Died Feb. 12, 1857.
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10). Born August 27, 1803. Died February 10, 1861.
Rogers, Henry D., Glasgow, Scotland (1). Born 1809. Died May 29, 1866.
Rogers, James B., Philadelphia, Pa. (1). Born February 22, 1803. Died June 15, 1852.
Rogers, William Barton, Boston, Mass. (1). Born Dec. 7, 1804. Died May 30, 1882.
Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Sager, Abram, Ann Arbor, Mich. (6). Born December 22, 1810. Died August 6, 1877.
Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.
Schley, William, New York, N. Y. (28). Died in 1882.
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Senter, Harvey S., Aledo, Ill. (20). Died in 1875.
Seward, William H., Auburn, N. Y. (1). Born May 16, 1801. Died Oct. 10, 1872.
Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.
Sherwin, Thomas, Dedham, Mass. (11). Born March 26, 1799. Died July 23, 1869.
Silliman, Benjamin, New Haven, Conn. (1). Born August 8, 1779. Died November 22, 1864.
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12).
Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec.
Smith, J. V., Cincinnati, Ohio (5). [26, 1880.
Smith, James Y., Providence, R. I. (9). Born September 15, 1809. Died in 1876.
Smith, Lyndon A., Newark, N. J. (9). Born November 11, 1795. Died December 15, 1865.
Snell, Ebenezer S., Amherst, Mass. (2). Born October 7, 1801. Died in September, 1877.
Sparks, Jared, Cambridge, Mass. (2). Born May 10, 1789. Died March 14, 1866.
Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.
Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.
Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.
St. John, Joseph S., Albany, N. Y. (28). Died Nov. 23, 1882.
Sullivant, Wm. S., Columbus, Ohio (7). Born Jan. 15, 1803. Died April 30, 1873.
Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born Jan. 20, 1778. Died Oct. 3, 1853.

Taylor, Richard C., Philadelphia, Pa. (1). Born January 18, 1789. Died November 26, 1851.

Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.

Teschemacher, J. E., Boston, Mass. (1). Died Nov. 9, 1853, aged 63.

Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.

Thompson, Alexander, Aurora, N. Y. (1).

Thompson, Zadock, Burlington, Vt. (1). Born May 23, 1796. Died Jan.

Thurber, Isaac, Providence, R. I. (9). [19, 1856.

Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.

Tolderoy, James B., Fredericton, N. B. (11).

Torrey, John, New York, N. Y. (1). Born August 15, 1796. Died March 10, 1873.

Torrey, Joseph, Burlington, Vt. (2). Born Feb. 2, 1797. Died Nov. 26, 1867.

Totten, Joseph G., Washington, D. C. (1). Born August 23, 1788. Died April 22, 1864.

Townsend, Howard, Albany, N. Y. (10). Died in 1867.

Townsend, John K., Philadelphia, Pa. (1).

Townsend, Robert, Albany, N. Y. (9). Died in 1866.

Troost, Gerard, Nashville, Tenn. (1). Born March 15, 1776. Died Aug. 14, 1850.

Tuomey, Michael, Tuscaloosa, Ala. (1). Born September 29, 1805. Died March 20, 1857.

Tyler, Edward R., New Haven, Conn. (1). Died Sept. 28, 1848.

Vancleve, John W., Dayton, Ohio (1).

Vanuxem, Lardner, Bristol, Pa. (1).

Vaux, William S., Philadelphia, Pa. (1). Born May 19, 1811. Died May 5, 1882.

Wadsworth, James S., Genesee, N. Y. (2). Born October 30, 1807. Died May 8, 1864.

Wagner, Tobias, Philadelphia, Pa. (9).

Walker, Joseph, Oxford, N. Y. (10).

Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.

Walker, Timothy, Cincinnati, Ohio (4). Born Dec. 1, 1802. Died Jan. 15, 1856.

Walsh, Benjamin D., Rock Island, Ill. (17).

Wanzer, Ira, Brookfield, Conn. (18). Born April 17, 1796. Died March 5, 1879.

Warren, Gouverneur Kemble, Newport, R. I. (12). Died Aug. 8, 1882, in his 64th year.

Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.

Watertown, Charles, Wakefield, Eng. (1).
Watkins, Samuel, Nashville, Tenn. (26).
Watson, J. Craig, Ann Arbor, Mich. (13). Born Jan. 28, 1838. Died Nov. 23, 1880.
Webster, Horace B., Albany, N. Y. (1). Died Dec. 8, 1843.
Webster, J. W., Cambridge, Mass. (1). Died Aug. 30, 1850, aged 57.
Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.
Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.
Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.
Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.
Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.
Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.
White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.
Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.
Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.
Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.
Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.
Willard, Emma, Troy, N. Y. (15). Born Feb. 23, 1787. Died April 15, 1870.
Williamson, R. S., San Francisco, Cal. (12).
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5). Born Feb. 6, 1826. Died June 11, 1875.
Woodbury, Levi, Portsmouth, N. H. (1). Born Dec. 22, 1789. Died Sept. 4, 1851.
Woodman, John S., Hanover, N. H. (11). Born in 1819. Died May 15, 1871.
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.
Young, Ira, Hanover, N. H. (1). Died Sept. 14, 1858, aged 57.

ADDRESS

BY

PROFESSOR GEORGE J. BRUSH,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

A SKETCH OF THE PROGRESS OF AMERICAN MINERALOGY.

MR. PRESIDENT, AND FELLOW MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

THE change in the Constitution effected at our last meeting, extending the scope of the Association and dividing it into nine sections, each with a Vice President, whose duty it is to deliver an address to the section over which he presides, has relieved the retiring President from attempting a general review of the progress of science during the past year.

I turn, therefore, to a more special subject, and invite your attention this evening to a sketch of the progress of American Mineralogy, since the commencement of this century, with particular reference to the labors of some of the early workers in the science on this continent.

During the last quarter of the eighteenth century, while great activity existed and rapid advance was made in the study of chemistry and mineralogy in Europe, almost nothing was accomplished in this new country. It is true that students in other departments of science, especially members of the medical profession, in the cities of Philadelphia, New York and Boston, attempted to arouse an interest in mineralogy, believing that the diffusion of a knowledge of this science would be of the utmost importance in the material development of the country. There were, however, no text-

books to aid the inquirer. There were no collections of minerals to stimulate the student. In the absence of these it was almost impossible that an interest in this science should be fostered, or that a spirit of investigation should be awakened.

As the first distinct beginning of the science, I may mention an association formed in 1798 in the city of New York, which assumed, as they expressed it, "the name and style of the American Mineralogical Society[1]. It announced as its object "the investigation of the Mineral and Fossil bodies which compose the Fabric of the Globe, and more especially for the Natural and Chemical History of the Minerals and Fossils of the United States." The distinguished Dr. Samuel Latham Mitchill, who seems to have been a man of universal genius, was at once its first president, its librarian, and its cabinet-keeper. The committee of the society issued a circular, in which, while expressing themselves, "desirous of obtaining and diffusing by every means in their power a correct and extensive knowledge of the mineral treasures of their country, they earnestly solicited their fellow-citizens to communicate to them on all mineralogical subjects, but especially on the following," viz.:

"1. Concerning the stones suitable to be manufactured into *gun-flints:* where are they found? and in what quantity? 2. Concerning *native brimstone or sulphur* or the waters or minerals whence it may be extracted? 3. Concerning *salt-petre:* where (if at all) found native? or the soils which produce it in the United States? 4. Concerning mines and ores of *lead:* in what places? the situation? how wide the vein? and in what kind of rock it is bedded."

This warlike demand seems to call more for the discovery of the materials for national defence than for the advancement of science, and besides being a commentary on the spirit of the times, gives a rather humorous impression of their strangely inadequate conception of the science of mineralogy, and its possible bearings on practical life. But in justice to them I should add that it is further announced that "specimens of ores, metals, coals, spars, gypsums, crystals, petrifactions, stones, earths, slates, clays, chalks, limestones, marbles, and every fossil substance that may be discovered or fall in the way of a traveller, which can throw light on the mineralogical history of America, will be exam-

[1] *The Medical Repository*, Vol. II, p. 114 (1799), edited by S. L. Mitchill and Edward Miller.

ined and analyzed without cost, sufficient pieces, with the owner's leave, being reserved for placing in the society's collection." I have quoted the circular almost verbatim to give you some idea of the genuine though crude longings for knowledge felt by our early mineralogists, and also of the generous spirit in which they worked.

A still more forcible picture of the ignorance of the time is given by the elder Professor Silliman in 1818:[2] "Notwithstanding the laudable efforts of a few gentlemen," he says, "to excite some taste for mineralogy, so little had been effected in forming collections, in kindling curiosity and diffusing information, that only fifteen years since (1803), it was a matter of extreme difficulty to obtain *among ourselves* even the *names* of the most common stones and minerals; and one might inquire earnestly and long before he could find any one to identify even *quartz*, *feldspar* or *hornblende* among the simple minerals, or *granite*, *porphyry* or *trap* among the rocks. We speak from experience, and well remember with what impatient, but almost despairing curiosity we eyed the bleak, naked ridges which impended over the valleys and plains that were the scenes of our youthful excursions. In vain did we doubt that the glittering spangles of mica, and the still more alluring brilliancy of pyrites, gave assurance of the existence of the precious metals in those substances, or that the cutting of glass by the garnet, and by quartz, proved that these minerals were the diamond; but if they were not precious metals, and if they were not diamonds, we in vain inquired of our companions, and even of our teachers, what they were."

Such, then, was the state of knowledge in mineralogy here at the commencement of the century. A few American minerals, collected by travellers from time to time, had before this been taken to Europe for identification, but among these were discovered only two minerals new to science. The Moravian missionaries found at St. Paul in Labrador, the beautiful species of feldspar called by Werner *labrador-stein*, which in more modern times we know under the name of *labradorite*. Klaproth, the most eminent analytical chemist of his time, discovered that the so-called fibrous barytes from Pennsylvania was the sulphate of the then newly discovered earth strontia.[3] He thus, for the first time, identified

[2] Review of Cleaveland's Mineralogy, *American Journal of Science* (1), I, p. 36.
[3] *Beiträge zur chemischen Kentniss der Mineralkörper*, II, 92.

the mineral species *celestite* which was subsequently found in various localities in Europe.

Although little had been accomplished in America previous to 1800, the first quarter of the new century was destined to show great development here in the study of mineralogy. During the early years of this quarter several collections of European minerals were brought to this country by American gentlemen who had availed themselves during a residence in Europe of the best opportunities for acquiring a knowledge of the science from the great masters of the subject in Germany and France. About this time also several colleges in the country had instituted chairs of chemistry and mineralogy, and a commencement was thus made in teaching these sciences in the higher schools. As the result of these influences the number of persons interested in mineralogy was largely increased, and an active search for minerals was initiated throughout all of the older United States and to a considerable extent also in Canada.

So energetically were these explorations followed up that in 1825 a Catalogue[4] of American minerals was published by Dr. Samuel Robinson, with their localities arranged geographically, and giving only such as were known to exist in the United States and the British Provinces. It formed an octavo volume of over three hundred pages.

That much credit was due to *many* workers during this period, both in the field and in the laboratory, there can be no question, but among them all I find four men standing forth so prominently as leaders, that I have thought it would be well for us to recall briefly something of the character of these men and their labors for the advancement of mineralogy in this country.

First among these I will mention Dr. ARCHIBALD BRUCE.[5] He was the son of Dr. William Bruce, a surgeon in the British army, and was born in New York in 1777. He was graduated at Columbia College; subsequently studied medicine, and in 1798 went to Edinburgh where, in 1800, he obtained his doctor's degree from that university. He was early interested in natural science and while still in college found, his biographer says, "the collection and examination of minerals — a pursuit not then at all attended

[4] *Catalogue of American Mineral Localities*, by Dr. Samuel Robinson. 8vo, pp. 316. Boston, 1825.

[5] For a biographical notice of Dr. Bruce see *American Journal of Science*, Vol. I, p. 299, 1818.

to in this country — was his particular relief from other studies; for even during his recreation he was ever on the lookout for something new or instructing in mineralogy."

When he went to Europe he took with him a large number of American minerals, and through exchanges with institutions and prominent mineralogists abroad, he established friendly relations with those most interested in his favorite science. After the completion of his medical studies, he travelled for two years on the continent of Europe, making the acquaintance of the Abbé Haüy, and other eminent mineralogists, and collecting an extensive cabinet of valuable minerals, which on his return to this country in 1803, he brought with him to New York. This collection, with another brought to New York about the same time by Mr. B. D. Perkins — both being made fully accessible to all interested in seeing them — contributed, it was said, more than any agencies had ever done before, to excite in the public an active interest in the science of mineralogy. Besides this Dr. Bruce entered into extensive correspondence with others interested in the subject, was active in visiting and discovering new mineral localities, and in advising, encouraging and inspiring young mineralogists. Finally, after well considering the matter, he established the first purely scientific periodical ever published in America. This was called the *American Mineralogical Journal*,[6] and the first number of it was published in 1810. It contained original contributions, chiefly on mineralogy, from a number of investigators. "It was received," says the elder Silliman, "in this country and in Europe in a flattering manner; it excited at *home* great zeal and effort in support of the sciences which it fostered, and *abroad* it was hailed as the harbinger of our future exertions." But, alas! it was in advance of the age, and, after struggling for several years, was given up on the publication of the fourth number. Possibly it would have continued longer had it not been for the failing health of its founder.

This Journal contained several important papers by Dr. Bruce: among them, the investigation and description of two new mineral

[6] The *American Mineralogical Journal*, being a collection of facts and observations tending to elucidate the mineralogy and geology of the United States of America; together with other information relating to mineralogy, geology and chemistry derived from scientific sources. Conducted by ARCHIBALD BRUCE, M.D., Professor of Materia Medica in the Medical Institution of New York, and Queen's College (N. J.). Vol. I, 270 pp. New York. 1814.

species, the *native magnesia* of Hoboken and the *red zinc oxide* of Sussex Co., New Jersey. These are the first American species described by an American mineralogist. So thoroughly was the work done by Bruce, that these species remain to-day essentially as he described them, and his papers may well be studied by mineralogists now as models of accuracy and clearness of statement.

Dr. Bruce did much also for the elevation of the medical profession, was one of the founders of the New York Medical Society, and was largely influential in obtaining the charter of the College of Physicians and Surgeons, in which he was subsequently the Professor of Materia Medica and Mineralogy.

He is described as a successful teacher, a man of wide acquirements and of great integrity, which qualities, with his abounding generosity and hospitality, commanded the respect and regard of all who knew him. It was a great loss to science and to his country that so able an investigator should have been cut off at the early age of 42. He died in New York, Feb. 24, 1818.

I have mentioned that the importation and exhibition of collections of minerals from Europe had contributed much to excite an interest in the study of mineralogy. It was necessary to have known minerals for study and comparison in order properly to determine those obtained by exploration here. In 1805, Colonel GEORGE GIBBS of Rhode Island, for many years a resident in Europe, returned from his travels with a collection of minerals the most extensive and valuable ever brought to America. Colonel Gibbs was a zealous cultivator of mineralogy and, fortunately for science, a young man of wealth. He used his money freely for the purchase of whole cabinets, as well as in personal explorations in search for minerals.

The larger part of his collection was made by the purchase of two famous European cabinets, one from the heirs of Gigot d'Orcy, a noted French collector, and said to have been the result of forty years' labor, the other from Count Gregoire de Razamowsky, a Russian nobleman, long resident in Switzerland. D'Orcy's cabinet numbered over 4,000 specimens, chiefly from France, Germany, Italy and Great Britain; Razamowsky's contained about 6,000 specimens from the Russian Empire and the remainder principally from Germany and Switzerland; in all, with the other col-

lections made by Colonel Gibbs, it is said that more than 20,000 specimens were brought by him to this country.[7]

In 1807, a portion of this collection was opened in Newport, and many interested in mineralogy made pilgrimages there, to view the treasures it contained. Among others was Professor Silliman, who states in his diary, that he spent many weeks in studying the minerals with Colonel Gibbs, finding in the latter "a scientific friend and a professional instructor and guide." That Colonel Gibbs reciprocated Professor Silliman's feelings of friendship there can be no doubt; for, after various offers to deposit his collection for exhibition in Boston, New York and elsewhere, to the great surprise of Professor Silliman, he proposed to open the cabinet at Yale College, provided rooms should be fitted up for its reception.

The proposition was promptly responded to by the authorities of the college, and in 1810, 1811 and 1812, under the personal supervision of Colonel Gibbs, it was opened and arranged, and generously placed at the disposition of the institution and the public. The opening of this collection in New Haven formed an important epoch in the history of the college, and gave a powerful impetus to science throughout the country. It was not only studied by the pupils of the college, but it was visited by travellers from all parts of the United States.

In 1825, the collection had for fifteen years been exhibited without any advantage to the owner, other than the satisfaction of observing the great amount of good which was effected by the knowledge it disseminated, and the enthusiasm with which it inspired students. Colonel Gibbs then offered the whole for sale, giving the college the preference as purchaser. Fortunately and mainly through the influence of Professor Silliman, the institution succeeded in raising the funds ($20,000) necessary for its purchase, and the ownership of this collection has exercised a most important influence in the development of natural science at New Haven.

Colonel Gibbs, however, did not confine himself to the collection of minerals in Europe. On his return to this country he made extensive journeys and opened up new mineral localities, giving his time and specimens freely to aid others who were interested in this special study. At Yale, as an incentive to stu-

[7] *Medical Repository* (2), V, 213 (1808).

dents, he for many years offered prizes for superiority of attainments in mineralogical knowledge and for services rendered to the science by useful discoveries and observations.[8]

He published valuable papers both in the *American Mineralogical Journal* and the *American Journal of Science*, and did much by his counsel and coöperation to support these publications. Indeed, it was from Colonel Gibbs that Professor Silliman first received the suggestion that he should institute a new Journal of Science, in order that the advantages already gained by the short-lived Mineralogical Journal might be secured and further progress for science might be made.[9]

In every way Colonel Gibbs proved himself a liberal patron of science, and it was most fortunate for the promotion of mineralogy in this country that he should so unselfishly have devoted his wealth and his personal influence to its advancement. He died August 5, 1833, aged 57.

Much as had been accomplished by the free exhibition of cabinets and the explorations and investigations of enthusiastic workers in mineralogy during the years from 1805 to 1815, a great drawback was now felt to the progress of the science from the want of text-books. Most of the literature of the subject was in German and French, but the works of the French and German authors had not then been translated and consequently were accessible only to the few who were acquainted with these languages.

In English there were not many treatises on the subject. That by Richard Kirwan, the eminent Irish mineralogist of the last century, was a renowned work in its day, but, as the last edition of it had been published in 1794, it was already too old to be of much service to the student. Jameson's treatise was somewhat more recent (1804),[10] but its great fulness and exclusive devotion to the Wernerian system made it an undesirable book for beginners, aside from the fact that its price was such that few students in those days could afford to buy it. So much progress had been made at home and abroad, that a work was needed here which should include the modern discoveries, and one also which should

[8] *American Mineralogical Journal*, p. 269.

[9] Obituary notice of Colonel Gibbs, by Professor Silliman, *American Journal of Science* (1), XXV, 214.

[10] Of Jameson's *Mineralogy*, 1st edition, the 1st volume was published in 1804, 2nd volume in 1805, 3d volume in 1808.

gather up the scattered facts already published in regard to American minerals.[11]

Fortunately for the further progress of science in this country this was done by Professor PARKER CLEAVELAND. His work was published in 1816, and was entitled "An Elementary Treatise on Mineralogy and Geology."[12]

Professor Cleaveland was professor of mathematics and natural philosophy in Bowdoin College, and, like many other professors of science in the early history of American colleges, was charged by the trustees to lecture also on mineralogy and chemistry. He was an enthusiastic student of mineralogy, was well acquainted with the literature of the science in various languages, had been a successful teacher of the subject for many years, and withal was both an explorer and investigator, and held intimate relations with the leading mineralogists of the day. The work was modelled on the general plan of Brongniart, combining the excellences of both the French and German schools, and gave in detail almost everything then known in regard to American minerals. It supplied the pressing need for a thorough, systematic and *American* treatise on mineralogy, well suited to all classes of students, and it was written in such a masterly style that it won for its author the highest praise from the leading mineralogists of the world. "It brought," says Professor Silliman, "within the reach of the American student the excellences of Kirwan, Jameson, Haüy, Brochant, Brongniart and Werner, and we are not ashamed," he says, "to have this work compared with those of these celebrated authors."[13] His biographer[14] states that "he received letters of respect and congratulation from Sir David Brewster, Sir Humphrey Davy and Dr. Macculloch, in England, from Berzelius, in Stockholm, Germar of Halle, from Brongniart, Baron Cuvier and the Abbé Haüy, in Paris."

[11] The *Manual of Mineralogy*, by A. AIKIN, was reprinted in Philadelphia in 1815, but it contained nothing in regard to American minerals.

[12] An elementary treatise on Mineralogy and Geology, being an introduction to the study of these sciences, and designed for the use of pupils,—for persons attending lectures on the subjects,—and as a companion for travellers in the United States of America. By PARKER CLEAVELAND, Professor of Mathematics and Natural Philosophy, and Lecturer on Chemistry and Mineralogy in Bowdoin College. One volume, 668 pp. 8vo, with six plates. Boston, 1816.

[13] Review of Cleaveland's Mineralogy, loc. cit.

[14] The main facts as stated in regard to Professor Cleaveland are from an address on his life and character by Leonard Woods, D. D., President of Bowdoin College. Brunswick, 1860.

The work at once took rank as one of the leading authorities on the science and was introduced as a class-book in the principal schools and colleges in America. The first edition was soon exhausted and a new and revised edition with more than a hundred pages of new matter was published in 1822. The demand was so great that this likewise was soon out of print and a third edition was called for by the public, but Professor Cleaveland had about this time become so engrossed in the administration of the affairs of the new Medical School at Brunswick that he was unable to respond to the call, having turned his thoughts and efforts in new directions.

Unfortunately for the science of mineralogy, in which he had obtained such eminence as an author and teacher, he no longer contributed actively to its progress, although he continued his work as lecturer on the science so long as he lived. He died Oct. 16, 1859, in the seventy-ninth year of his age.

The last to be mentioned of these early leaders is Professor BENJAMIN SILLIMAN. His name is so intimately associated with the progress of science on this continent during the first half of the present century, that his life work is more or less familiar to all[15]. But the important service he rendered in the early history of mineralogy deserves especial recognition here, not only for the work he himself did in the laboratory and the field, but because his enthusiasm and zeal were a constant inspiration to others.

Commencing with the historic "candle-box" of unlabelled stones which he took to Dr. Adam Seybert, of Philadelphia, to be named, he began with enthusiasm the acquisition of knowledge and the gathering of material to illustrate the mineral kingdom. During a residence in England and Scotland in 1805–6 he had opportunities to add to his information, and collect many specimens, chiefly from the mines of Derbyshire and Cornwall. On his return to America he at once applied the knowledge he had acquired in making an exploration of the mineral structure of the environs of New Haven, and read a paper on this subject to the Connecticut Academy of Arts and Sciences in September, 1806.

In the following year he induced the corporation of the college

[15] See Life of BENJAMIN SILLIMAN, M. D., LL. D., Late Professor of Chemistry, Mineralogy and Geology in Yale College. Chiefly from his manuscript reminiscences, diaries, and correspondence, by GEO. P. FISHER, Professor in Yale College. Two volumes, 12 mo. New York, 1866.

to purchase the mineral collection of Mr. B. D. Perkins of New York (already referred to), for one thousand dollars, thus placing the institution in possession of means for illustrating the science of mineralogy far in advance of anything it had before enjoyed.

The occurrence of the fall of the Weston Meteorite in December, 1807, offered an opportunity for Professor Silliman to undertake, in connection with his colleague, Professor Kingsley, an investigation into the circumstances of the phenomena, and the character of the stones which fell at that time. The results of this investigation were presented to the American Philosophical Society and published in the *American Philosophical Transactions* in 1809. The diligence employed in obtaining all the facts possible from eye-witnesses of the occurrence, and the care and skill shown in the chemical and mineralogical examination of the meteorite, made this paper one of the most remarkable memoirs of the time, and attracted the attention of philosophers throughout the world.

As already stated, it was the personal enthusiasm and magnetic influence of Professor Silliman which led Colonel Gibbs to deposit his great cabinet of minerals in New Haven, under the care of his friend. It was due to the same qualities in Professor Silliman that the college secured the permanent possession of this invaluable collection, which probably has done more than any other single agency to create an interest in and disseminate a knowledge of mineralogy in this country.

The establishment of the *American Journal of Science* in 1818, now everywhere recognized as of inestimable value to all departments of science, was peculiarly helpful to mineralogy, and the early volumes are rich in articles on this subject. Professor Silliman's original contributions to science were more in chemistry and geology, but he also is the author of several important papers on mineralogy, and was the discoverer of the occurrence of native tungstic acid as a mineral species. For more than fifty years he continued as a teacher in Yale College, and when he resigned his professorship in 1853, he had the satisfaction to have as his successor in the department of mineralogy and geology Professor James D. Dana, who was already among the foremost mineralogists of the day, and whose published works, before and since his accession to this professorship, have done so much for the advancement of mineralogy.

Professor Silliman retired from his active labors in his seventy-fourth year, still in full possession of his remarkable physical and mental powers, and lived honored and revered until Nov. 24, 1864, when he passed to his rest.

It will be inferred from what has been said of these pioneers that the developments and discoveries of minerals, during the first twenty-five years of the century, were due entirely to individual enthusiasm and private enterprise. Up to this time no aid had been received from either state or national governments, and in looking over the work accomplished during this period we are filled with wonder and admiration at the energy and rare devotion to science exhibited. The larger portion of the continent was an unbroken wilderness, and the facilities of communication even in the settled parts of the country were of the most primitive character. Yet at the present day, with our means of rapid transportation, many naturalists would hesitate to undertake the long journeys then made for purely scientific purposes.

Geologists as well as mineralogists will recall how much science is indebted to such men as William Maclure, James Pierce, Thomas Nuttall (the botanist), and others who made extensive trips through the whole territory east and in some instances to the west of the Mississippi River. Maclure not only devoted his time and money to making and publishing a geological survey of the United States and Canada, the first report of which was made in 1809,[16] but to him the Academy of Natural Sciences, in Philadelphia, owes its first endowment.[17]

I shall be pardoned, I trust, if I mention still another signal instance of private liberality in this connection. Gen. Stephen Van Rensselær, of New York, a generous patron of science, defrayed all the expenses of a geological survey of the country adjacent to the Erie Canal, including the making of a geological section from Lake Erie to the eastern coast of Massachusetts.[18] This survey

[16] Published later with additions, in a volume of 127 pp., 8vo, with two plates. Philadelphia, 1817.

[17] Memoir of William Maclure, Esq., late President of the Academy of Natural Sciences of Philadelphia, by S. G. Morton, M. D. *Am. J. Sci.*, (1) XLVII, 1 (1844.)

[18] A Geological and Agricultural Survey of the district adjoining the Erie Canal, in the State of New York, taken under the direction of Hon. Stephen Van Rensselær. Part I, containing a description of the rock formations, together with a profile extending from the Atlantic to Lake Erie. 163 pp. 8vo. Albany, 1824.

was under the charge of Professor Amos Eaton, with a competent corps of assistants, and was continued for four years, from 1820 to 1824, at a cost of many thousands of dollars. General Van Rensselær was also the founder of the first school of technical science in this country—the Rensselær Polytechnic Institute, at Troy, which was placed under the charge of Professor Eaton. It may be interesting here, in these days of summer schools, to recall, although parenthetically, that what was probably the first summer school of science in the United States was established more than fifty years ago in connection with this institution. The school consisted of a flotilla of towed canal boats, and the route was from Troy to Lake Erie. It took two months for the trip and all important points on the way were visited. Instruction by lectures and examinations was given in mineralogy, geology, botany, zoology, chemistry, experimental philosophy and practical mathematics, particularly land surveying, harbor surveying and engineering. One of the largest boats in the flotilla was fitted up as a laboratory, with cabinets in mineralogy and geology and also scientific books for reference. Students were taught the method of procuring specimens and were required to make collections of whatever was interesting on the route.[19]

The public mind was finally awakened to the importance of the work which these explorers and investigators had carried on single handed. Government now came to the aid of Science. In 1824, one State legislature, that of North Carolina, authorized a geological survey to be made. This example was followed in 1830 by Massachusetts and soon after by New York, Pennsylvania, Virginia and other states, and also by the national government, until as is now well known, the whole territory of the United States and Canada, either has been or is in the process of being surveyed. Several of the state surveys published independent volumes on the mineralogy of their respective states, and these surveys have been a powerful auxiliary in extending our knowledge of the occurrence of minerals on this continent. The opening of mines and quarries throughout the country has also furnished abundant material for study. The large number of original contributions which have been published in the volumes of State Surveys, the treatises by American authors, and the still larger number of memoirs and papers com-

[19] *American Journal of Science* (1), XVIII, 200. (1830.)

municated to our academies of science and scientific journals cannot be even enumerated in this place, neither is it my purpose to attempt to give here a list of the names of those who have been actively engaged in making researches on American minerals.[20] Still less can I attempt to give an account of the work that has been and is being done by living mineralogists. The sketch which I have presented of the four typical workers has in a measure shown the character of our early mineralogists, the earnest spirit in which they labored, and what they accomplished in the first quarter of the century. The point to which the science has reached in the last quarter of the century cannot be unfamiliar to you all.

In the time that remains I desire to call your attention to some of the developments made in the field in which our mineralogists have worked. It was thought by many scientists in the first half of this century that our rocks seemed likely to afford less variety of mineral contents than the rocks of Europe. Further study, however, and more careful and extended observations encourage us to believe that our mineral riches, even in variety of species, will compare favorably with those of other continents. Already fully one-half of the known mineral species have been found here. The present number of known minerals is variously estimated to be from seven hundred to one thousand. There have been described, as occurring here, nearly three hundred supposed new American minerals. Of these, perhaps one-quarter are new to science and the remainder have either been proved to be identical with species already described, or their characters are so imperfectly given that further investigation is needed to ascertain what they are.

Among these new minerals are some of great interest to science. Time, however, will not allow, even if your patience would permit me, to give the facts in detail; but in justice to the describers of those announced to be new, I will print, as an appendix to this address, as complete a list as I have been able to make of the names of the proposed new American mineral species, with the names of their sponsors. The list will, I trust, be instructive both as a warn-

[20] For a list of papers by American chemists, including a large proportion of papers published in mineralogy, see *American Contributions to Chemistry*, an address delivered on the occasion of the celebration of the Centennial of Chemistry at Northumberland, Pa., August 1, 1874, by BENJAMIN SILLIMAN. 175 pp. 8vo. Reprinted from the *American Chemist* for August, September and December, 1874.

ing and an encouragement to investigators. The ambition to make new species is recognized as a drawback in every department of science, and mineralogy has probably not suffered in this respect more than other sciences. Nor do I believe that American mineralogists have, as a class, been less careful in describing new species than their European confrères. There have been flagrant examples of carelessness in both hemispheres, and the growing tendency during the last ten or fifteen years to call things new which have been only imperfectly investigated cannot be too strongly censured. "If two very simple rules," says a recent writer,[21] "could be conscientiously followed by those investigating supposed new mineral species, the science of mineralogy would be vastly benefited. These are: first, that the material analyzed should in every case be proved by a careful microscopical and chemical examination to be *homogeneous;* and second, that the thorough investigation which is to establish the position of a "new species" should precede, not follow, the giving of a new name. A mineral which can only be partially described does not deserve a name.

In comparing the minerals found in America with those of Europe, although interesting minor variations are observed, it can hardly be expected that very marked differences should exist. This is, of course, due to the fact, that in the inorganic kingdom, nature has everywhere to do with the same elements, under essentially like conditions. A large number of remarkable analogies between the minerals of the two continents will occur to any one familiar with the subject, as, for example, the character of the occurrence of individual minerals in the rocks of the northeastern United States and Canada as compared with those of Norway and Sweden, and numberless instances of like association of minerals in various parts of Europe find their counterparts here.

A marked feature of American minerals is the grand scale upon which crystallization has taken place, individual crystals of large size being very common. The granite veins of New England afford striking examples of this kind. We have common mica, in sheets a yard across; feldspar has been observed where a single cleavage plane measured ten feet; gigantic hexagonal prisms of beryl, four feet long and more than two feet in diameter, and

[21] Preface to the Third Appendix to the fifth edition of Dana's *Mineralogy*. New York. 1882.

weighing over two tons, have been described; spodumene crystals six to seven feet in length and a foot or more across, and masses of rock crystal of immense size, have been found. Canada and New York have given crystals of apatite, phlogopite and sphene which for these species are of marvellous grandeur in dimensions. Many other American localities might be mentioned where giant crystals occur. While it is true that these are extraordinary instances, it is also true as a general fact, common to a very large proportion of the minerals found in this country, that the species occur in much larger crystals than those obtained from European localities.

Another point worthy of note is the occurrence in comparatively large quantities, and over wide areas, of some of the rarer elements as constituents of the minerals found. In illustration of this we have, among the rare earths, *glucina* combined with silica and alumina in the mineral *beryl*, occurring in large quantity and perhaps in a hundred or more places; *zirconia*, in the mineral *zircon*, is also very widespread in its range of occurrence as an original constituent of the older rocks, as well as a vein mineral; localities are known which have furnished this rare species by the hundred weight. The *cerium* earths are found largely in the mineral *allanite*, which occurs in so many places that it may be said to be a common mineral in the United States. These earths are also found in the rare phosphate *monazite*, a mineral that in America has a wide range of localities, and recently this species has been found in crystals of two, three, and, in one instance, of eight pounds in weight. Again, three new earth metals, *mosandrum*, *phillipium* and *decipium* have been described as occurring with the cerium earths and yttria in the North Carolina *samarskite*. The rare alkali metal *lithium*, sometimes associated with the still rarer metals *rubidium* and *cæsium*, is found not only of widespread occurrence in our lithia micas, but the mineral *spodumene*, containing from five to eight per cent of lithia, occurs by the ton in at least one locality and must be looked upon as one of the common American minerals, being found in the granite veins in Maine, New Hampshire, Massachusetts and Connecticut, and as far south as North Carolina and Georgia. Lithia also is one of the constituents of the phosphate *triphilite*, and there are several localities known where this mineral occurs abundantly. Again, we

have the frequent occurrence of some of the rare metals which form metallic acids: *Columbium*, the first metal, new to science, discovered in America, associated with its twin metal *tantalum*, is found in columbite in our granite veins from Maine to Georgia, a range of more than a thousand miles, in a score or more of places, and sometimes is obtained by the hundred weight at a single locality. The American variety of *samarskite*, another rare columbate, has also been found in masses of fifty pounds or more in weight, and these acids occur in still other American species. *Molybdenum*, both as sulphide and in the oxidized form as native molybdic acid and molybdate of lead, is found in many localities, and occasionally in large quantity. Quite recently *vanadium* compounds have been discovered in several places, and *tungstates* have also been observed over a wide range of country. *Titanium* has been found in enormous quantities in extensive deposits of titanic iron as well as in the form of rutile and in sphene. The rare metal *tellurium* occurs native in Colorado in one locality where single masses of twenty-five pounds in weight have been taken out, and several new tellurium compounds have been found in our western mines.

It is, perhaps, unnecessary to enumerate more fully the many occurrences of other rare elements in American minerals. Enough has already been said to show that important developments have been made in the discovery and investigation of the minerals found in our American rocks during the past eighty years. Nevertheless it is but a commencement in the work. Only a very small portion of our territory has been explored with any thoroughness, and none of it exhaustively. The enormous production of the precious metals, and the extensive deposits of ores of the more common metals which have been opened up during the past twenty or thirty years, have placed us in the front rank as metal producers, but we are still far behind Europe in the variety of minerals obtained from our mines. This may be due, in some instances, to the character of the veins or ore deposits, there being, as in many of our gold and silver mines, remarkably few associated minerals. In other cases, however, it is doubtless due to the fact that very few persons connected with our mines have even an elementary knowledge of the rudiments of mineralogy, while in continental Europe almost every mining officer is familiar with all

the ordinary minerals. Thanks to the training of our schools of science, an improvement in this respect is already noticeable, as is shown in the discoveries made in the mines of our Western States and Territories during the past few years.

While the service done for mineralogy by our geological surveys is gratefully acknowledged, we feel we have a right to demand much more from them in the future. Mineralogy has been too largely looked upon as a guide to the discovery of useful ores and minerals and not as a matter for scientific study; fortunately during the past decade the discoveries in optical mineralogy, and their importance in the determination of the constituent minerals of the crystalline rocks, have led many geologists to again recognize the desirability of a knowledge of our science. Much will be accomplished if those in charge of geological surveys will direct competent persons to make observations, not only on the main mineral constituents of rocks but also on the manner of occurrence of individual minerals. The careful inspection of quarries and mines is greatly to be desired. These are rich sources for minerals; but unless constant watchfulness is exercised, valuable material for science is in danger of being buried out of sight.

It is too true that many of the most interesting discoveries already recorded seem to have been due more to the result of fortunate accident than of systematic and intelligent exploration. If our trained mineralogists, instead of devoting most of their attention to the examination of specimens in cabinets collected by others, would give more time to personal observation in the field in the study of the order and manner of occurrence of mineral species in place, our knowledge would doubtless be greatly promoted. Again, if our wealthy amateurs could be induced to spend their money as freely in the exploration of promising American localities as in the importation of costly European specimens, we might hope for many important discoveries, and they could have the satisfaction not only of gaining novelties for their collections, but incidentally they would do much to foster science.

In order to keep pace with the progress of the science, we need many more workers who will devote themselves especially to mineralogical research, and we need more of the spirit of the early

workers. It is my belief that the number of persons at present interested in the study here, either as amateurs or investigators, is relatively less than in 1825. The mineralogy of to-day is a very different subject from the mineralogy of the commencement of the period over which we have so hastily glanced. Then the study of minerals was confined almost exclusively to their external characters. Led by Werner[22] and reinforced by his most gifted pupil Mohs, the majority of mineralogists claimed mineralogy to be a purely natural history science. They gave their attention, as has been well said, entirely to "*how the mineral looked*," and not at all to "*what it was*." On the other hand the development of analytical chemistry by the labors of Klaproth and Berzelius led many to take up mineralogy from a purely chemical standpoint. These two schools working independently brought great confusion into the science. The discoveries of Haüy in crystallography, and especially his labors in establishing a mathematical foundation for the geometrical form of crystals, and the recognition that the constancy of form depended on the constancy of the "integrant molecule," were steps which paved the way for modern mineralogy. In this a union of all the physical, geometrical and chemical properties is required in order to determine the true character of a mineral.

Further, we are called upon to investigate the history of its origin, its relation to associated species, the changes which it undergoes, and the causes and results of those changes. Here we have to do largely with both geology and chemistry. From this it becomes evident that a much broader foundation is now required for the mineralogist than in the early days of the century. The bearing of physics, geology and chemistry in the study of the mineral kingdom must be thoroughly recognized and appreciated by every investigator who desires to contribute to further progress. No mineralogist can expect to have a profound knowledge in all these directions, but he must be at least capable of intelligently applying to his subject the results obtained by experts in these sciences. Mineralogy is deeply indebted to

[22] Werner, although using physical characters in the main, and entirely in the determination of minerals, recognized chemistry in arranging minerals in families or groups, but Mohs, Jameson and others founded their systems "totally independent of any aid from chemistry." (Preface, Jameson's *Mineralogy*, 3d ed., Vol. I, p. iii.)

special investigators in all these departments. Without their co-operation it would have been impossible to discover the relations of form and other physical characters with that fundamental arrangement of molecules whose nature it is now admitted controls all the properties of a substance.

The study of natural crystals has yielded rich material for the physicist. In the department of optics it has given results from which many fundamental laws have been deduced; and natural crystals, too, have furnished, in many cases, the very apparatus which made investigations possible. Some chemists claim that mineralogy is not at all a science by itself, and constitutes only a small part of inorganic chemistry. It can be unquestionably conceded that a knowledge of chemistry is fundamental, and in consequence this claim has a certain plausibility. On the other hand, we contend that it was largely the labors of the mineralogists on the physical characters of minerals, and especially their demonstration of the relation of form to chemical composition which finally awakened chemists to a more profound study of their own subject. The law of isomorphism was discovered by a chemist, whose training as an expert crystallographer in the examination of natural crystals made it possible for him to recognize the wonderful relation of form to composition. Dimorphism was first established from observations made on minerals, and it is in the study of the mineral kingdom that the laws of isomorphism and dimorphism find abundant demonstration. From the further investigation of the chemical nature of minerals we may hope for new light on the molecular constitution of substances which as yet the chemist has been unable to reproduce. We have already indicated the interdependence of geology and mineralogy. May we not claim the same interdependence of mineralogy, physics and chemistry, letting each go on in its own sphere, contributing to the general progress, sure that every new fact observed and every new law discovered will be for the common advancement of all?

APPENDIX.

A List[23] *of Names given to Minerals from the United States and Canada, for the most part described as New Species.*

Acadialite (= Acadiolite, *T. Thomson*)	*F. Alger* and *C. T. Jackson.*
Adamsite	*C. U. Shepard.*
Aglaite	*A. A. Julien.*
Alaskaite	*G. A. König.*
Albertite	*C. Robb.*
Algerite	*T. S. Hunt.*
Ambrosine	*C. U. Shepard.*
Amesite	*C. U. Shepard.*
Animikite	*H. Wurtz.*
Annite	*J. D. Dana.*
Anthophyllite, hydrous	*T. Thomson.*
Apatoid (*m*)[24]	*C. U. Shepard.*
Aquacreptite	*C. U. Shepard.*
Aragotite	*F. E. Durand.*
Arkansite	*C. U. Shepard.*
Baltimorite	*T. Thomson.*
Barnhardite	*F. A. Genth.*
Barytocelestite (= baryto sulphate of strontia	*T. Thomson.*
Beaumontite	*A. Lévy.*
Begeerite	*G. A. König.*
Bernhardinite	*J. M. Stillman.*
Bismuthaurite	*C. U. Shepard.*
Blackmorite	*A. C. Peale.*
Boltonite	*C. U. Shepard.*
Bowenite	*J. D. Dana.*
Brucite (= Native Magnesia, *Bruce*)	*F. S. Beudant.*

[23] This list is compiled almost entirely from the *Fifth edition of Dana's Mineralogy, and Appendices I–III to the same work*, in which will be found references as to where these minerals were originally described, as also any further facts published in regard to them, and their present position in the science.

[24] (*m*) signifies that the substance was found in a meteorite.

Brucite (Chondrodite) . . . . . *G. Gibbs.*
Byerite . . . . . *J. W. Mallet.*
Bytownite . . . . . *T. Thomson.*

Calaverite . . . . . *F. A. Genth.*
Calcimangite . . . . . *C. U. Shepard.*
Calcozincite . . . . . *C. U. Shepard.*
Calstronbarite . . . . . *C. U. Shepard.*
Calyptolite . . . . . *C. U. Shepard.*
Campbellite (*m*) . . . . . *St. Meunier.*
Canaanite . . . . . *S. L. Dana.*
Cantonite . . . . . *N. A. Pratt.*
Carrollite . , ... *W. L. Faber.*
Cassinite . . . . . *I. Lea.*
Catlinite . . . . . *C. T. Jackson.*
Celestialite (*m*) . . . . . *J. L. Smith.*
Celestite (= schwefelsaurer Strontianit aus Pennsylvanien, *Klaproth*, 1797) . *A. G. Werner.*
Centrallasite . . . . . *H. How.*
Cerinite . . . . . *H. How.*
Chalcodite . . . . . *C. U. Shepard.*
Chalcophanite . . . . . *G. E. Moore.*
Chathamite . . . . . *C. U. Shepard.*
Chelmsfordite . . . . . *J. F.* and *S. L. Dana.*
Cherokine . . . . . *C. U. Shepard.*
Chesterlite . . . . . *T. F. Seal.*
Chiltonite . . . . . *E. Emmons.*
Chladnite (*m*) . . . . . *C. U. Shepard.*
Chlorastrolite . . . . . *C. T. Jackson* and *J. D. Whitney.*
Chlorophyllite . . . . . *C. T. Jackson.*
Chromchlorite . . . . . *R. Hermann.*
Chrysophane (= Seybertite) . . . . *A. Breithaupt.*
Cleavelandite . . . . . *H. J. Brooke.*
Cleiophane . . . . . *T. Nuttall.*
Clingmanite . . . . . *B. Silliman, Jr.*
Clinochlore . . . . . *W. P. Blake.*
Clintonite (= Seybertite) . . . . . *W. W. Mather.*
Coloradoite . . . . . *F. A. Genth.*
Columbite (= ore of Columbian, *C. Hatchett*, 1802) . . . . . *R. Jameson.*
Cookeite . . . . . *G. J. Brush.*
Copperasine . . . . . *C. U. Shepard.*
Coracite . . . . . *J. L. LeConte.*
Corundellite . . . . . *B. Silliman, Jr.*
Corundophilite . . . . . *C. U. Shepard.*
Cryophyllite . . . . . *J. P. Cooke.*
Cryptomorphite . . . . . *H. How.*

Culsageeite . . . . . *J. P. Cooke.*
Cummingtonite (= Hornblende) . . . *C. Dewey.*
Cummingtonite (= Rhodonite) . . . *C. F. Rammelsberg.*
Cuproscheelite . . . . . *J. D. Whitney.*
Cyanolite . . . . . *H. How.*
Cymatolite (= Cumatolite) . . . . . *C. U. Shepard.*
Cyrtolite . . . . . *W. J. Knowlton.*

Danaite . . . . . *A. A. Hayes.*
Danalite . . . . . *J. P. Cooke.*
Danburite . . . . . *C. U. Shepard.*
Daubréelite (*m*) . . . . . *J. L. Smith.*
Dawsonite . . . . . *B. J. Harrington.*
Delawarite . . . . . *I. Lea.*
Deweylite . . . . . *E. Emmons.*
Diabantite . . . . . *G. W. Hawes.*
Dickinsonite . . . . . *G. J. Brush* and *E. S. Dana.*
Dimagnetite . . . . . *C. U. Shepard.*
Ducktownite . . . . . *C. U. Shepard.*
Dudleyite . . . . . *F. A. Genth.*
Dysyntribite . . . . . *C. U. Shepard.*
Dysluite . . . . . *W. H. Keating.*
Dyssnite . . . . . *F. v. Kobell.*

Edenite . . . . . *E. F. Glocker.*
Edwardsite . . . . . *C. U. Shepard.*
Emerald Nickel . . . . . *B. Silliman, Jr.*
Emerite . . . . . *C. U. Shepard.*
Emmonite . . . . . *T. Thomson.*
Enceladite (= Warwickite, *Shepard*) . *T. S. Hunt.*
Eosphorite . . . . . *G. J. Brush* and *E. S. Dana.*
Eremite . . . . . *C. U. Shepard.*
Erilite . . . . . *H. C. Lewis.*
Erusibite . . . . . *C. U. Shepard.*
Erythrite . . . . . *T. Thomson.*
Euchlorite . . . . . *C. U. Shepard.*
Eucryptite . . . . . *G. J. Brush* and *E. S. Dana.*
Eumanite . . . . . *C. U. Shepard.*
Euphyllite . . . . . *B. Silliman, Jr.*
Eupyrchroite . . . . . *E. Emmons.*

Fairfieldite . . . . . *G. J. Brush* and *E. S. Dana.*
Ferroilmenite . . . . . *R. Hermann.*
Ferrotellurite . . . . . *F. A. Genth.*
Ferrotitanite (= Schorlomite) . . . *J. D. Whitney.*
Fillowite . . . . . *G. J. Brush* and *E. S. Dana.*
Fowlerite . . . . . *C. U. Shepard.*
Franklinite . . . . . *P. Berthier.*

Genthite (= Nickel-gymnite, *Genth*) . *J. D. Dana.*
Gibbsite . . . . . *J. Torrey.*
Glossecollite . . . . . *C. U. Shepard.*
Goshenite . . . . . *C. U. Shepard.*
Grahamite . . . . . *H. Wurtz.*
Gunnisonite . . . . . *F. W. Clarke* and *N. W. Perry*
Gymnite . . . . . *T. Thomson.*

Haddamite . . . . . *C. U. Shepard.*
Hallite . . . . . *A. R. Leeds.*
Harrisite . . . . . *C. U. Shepard.*
Hatchettolite . . . . . *J. L. Smith.*
Haydenite . . . . . *P. Cleaveland.*
Hebronite . . . . . *F. v. Kobell.*
Henryite . . . . . *F. M. Endlich.*
Hermannite . . . . . *A. Kenngott.*
Hermannolite . . . . . *C. U. Shepard.*
Hetærolite . . . . . *G. E. Moore.*
Hexagonite . . . . . *E. Goldsmith.*
Hiddenite . . . . . *J. L. Smith.*
Hitchcockite . . . . . *C. U. Shepard.*
Holmesite (= Holmite)
Holmite (= Seybertite) . . . . . *T. Thomson.*
Hortonite (= altered pyroxene). . . .
Hortonolite . . . . . *G. J. Brush.*
Houghite . . . . . *C. U. Shepard.*
Howlite . . . . . *J. D. Dana.*
Hudsonite . . . . . *L. C. Beck.*
Huebnerite . . . . . *E. Riotte.*
Huntilite . . . . . *H. Wurtz.*
Huronite . . . . . *T. Thomson.*
Hydrocuprite . . . . . *F. A. Genth.*
Hydrofranklinite . . . . . *W. T. Roepper.*
Hydromagnesite (pulverulent mineral from Hoboken, N. J., = magnesia-alba, *T. Wachtmeister*) . . . . . *F. v. Kobell.*
Hydroniccite . . . . . *C. U. Shepard.*
Hydronickelmagnesite . . . . . *C. U. Shepard.*
Hydrotitanite . . . . . *G. A. König.*

Ilesite . . . . . *A. F. Wuensch.*
Indianaite . . . . . *E. T. Cox.*
Iodolite (*m*) . . . . . *C. U. Shepard.*
Ionite . . . . . *S. Purnell.*

Jacksonite (= Anhydrous Prehnite) . *J. D. Whitney.*
Jefferisite . . . . . *G. J. Brush.*

Jeffersonite . . . . . *L. Vanuxem* and *W. H. Keating.*
Jenkinsite . . . . . *C. U. Shepard.*

Keatingine . . . . . *C. U. Shepard.*
Kerrite . . . . . *F. A. Genth.*
Küstelite . . . . . *A. Breithaupt.*

Labradorite (= Labradorstein, *Werner*) *J. C. Delamétherie.*
Lancasterite . . . . . *B. Silliman, Jr.*
Latrobite . . . . . *H. J. Brooke.*
Lawrenceite (*m*) . . . . . *A. Daubrée.*
Ledererite . . . . . *C. T. Jackson.*
Lederite . . . . . *C. U. Shepard.*
Leidyite . . . . . *G. A. König.*
Lennilite . . . . . *I. Lea.*
Lernilite (wrong orthography for Lennilite) . . . . . *A. Schrauf.*
Lesleyite . . . . . *I. Lea.*
Leucanterite . . . . . *C. U. Shepard.*
Leucaugite . . . . . *J. D. Dana.*
Lincolnite . . . . . *E. Hitchcock.*
Lintonite . . . . . *S. F. Peckham* and *C. W. Hall.*
Lionite . . . . . *T. Berdell.*
Lithiophilite . . . . . *G. J. Brush* and *E. S. Dana.*
Loganite . . . . . *T. S. Hunt.*
Louisite . . . . . *D. Honeymann.*
Loxoclase . . . . . *A. Breithaupt.*
Luckite . . . . . *A. Carnot.*

Maconite . . . . . *F. A. Genth.*
Macfarlanite . . . . . *A. H. Sibley.*
Maclurite (= Chondrodite) . . . . . *H. Seybert.*
Maclurite (= Pyroxene) . . . . . *T. Nuttall.*
Magnesia, native (= Brucite, *Beudant*) *A. Bruce.*
Magnolite . . . . . *F. A. Genth.*
Mallardite . . . . . *A. Carnot.*
Mangan-amphibole . . . . . *R. Hermann.*
Manganese, ferro-silicate, ... *T. Thomson.*
Manganese, sesqui-silicate (= Dyssnite, *v. Kobell*) . . . . . *T. Thomson.*
Marasmolite . . . . . *C. U. Shepard.*
Marcylite . . . . . *C. U. Shepard.*
Marionite . . . . . *W. Elderhorst.*
Mariposite . . . . . *B. Silliman, Jr.*
Marmolite . . . . . *T. Nuttall.*
Masonite . . . . . *C. T. Jackson.*
Melaconite . . . . . *J. D. Dana.*

Melan-Asphalt (= Albertite, *Robb*) . . *C. M. Wetherill.*
Melanolite . . . . . *H. Wurtz.*
Melanosiderite . . . . . *J. P. Cooke.*
Metacinnabarite . . . . . *G. E. Moore.*
Microlite . . . . . *C. U. Shepard.*
Molybdate of Iron . . . . . *D. D. Owen.*
Monrolite . . . . . *B. Silliman, Jr.*
Montanite . . . . . *F. A. Genth.*
Moronolite . . . . . *C. U. Shepard.*
Mullicite . . . . . *T. Thomson.*

Nacrite (= Muscovite) . . . . . *T. Thomson.*
Necronite . . . . . *H. H. Hayden.*
Nemalite . . . . . *T. Nuttall.*
Neurolite . . . . . *T. Thomson.*
Newportite . . . . . *J. G. Totten.*
Niccochromite . . . . . *C. U. Shepard.*
Nickel-gymnite (= Genthite, *Dana*) . *F. A. Genth.*
Nickel vitriol . . . . . *T. S. Hunt.*
Nuttallite . . . . . *H. J. Brooke.*

Ontariolite . . . . . *C. U. Shepard.*
Owenite . . . . . *F. A. Genth.*
Ozarkite . . . . . *C. U. Shepard.*

Paracolumbite . . . . . *C. U. Shepard.*
Parailmenite . . . . . *C. U. Shepard.*
Parathorite . . . . . *C. U. Shepard.*
Parophite . . . . . *T. S. Hunt.*
Pattersonite . . . . . *I. Lea.*
Paulite . . . . . *A. G. Werner.*
Peckhamite (*m*) . . . . . *J. L. Smith.*
Pelhamine . . . . . *C. U. Shepard.*
Pennite . . . . . *R. Hermann.*
Peristerite . . . . . *T. Thomson.*
Perthite . . . . . *T. Thomson.*
Philadelphite . . . . . *H. C. Lewis.*
Phlogopite . . . . . *A. Breithaupt.*
Phosphuranylite . . . . . *F. A. Genth.*
Phyllite . . . . . *T. Thomson.*
Phytocollite . . . . . *H. C. Lewis.*
Polyadelphite . . . . . *T. Thomson.*
Polylite . . . . . *T. Thomson.*
Prehnite, anhydrous, ... *J. D. Whitney.*
Priceite . . . . . *B. Silliman, Jr.*
Protovermiculite . . . . . *G. A. König.*
Psittacinite . . . . . *F. A. Genth.*

Pyromelane . . . . . *C. U. Shepard.*
Pyrophosphorite . . . . . *C. U. Shepard, Jr.*

Rahtite . . . . . *C. U. Shepard.*
Randite . . . . . *G. A. König.*
Raphilite . . . . . *T. Thomson.*
Rastolyte . . . . . *C. U. Shepard.*
Reddingite . . . . . *G. J. Brush* and *E. S. Dana.*
Remingtonite . . . . . *J. C. Booth.*
Rensselærite . . . . . *E. Emmons.*
Retinalite . . . . . *T. Thomson.*
Rhodophyllite . . . . . *F. A. Genth.*
Richmondite . . . . . *A. Kenngott.*
Rœpperite (= iron-manganese chrysolite, *Rœpper*, = Stirlingite, *Kenngott*) . *G. J. Brush.*
Rœpperite (= iron-manganese dolomite) *A. Kenngott.*
Rogersite . . . . . *J. L. Smith.*
Roscoelite . . . . . *J. Blake.*
Rutherfordite . . . . . *C. U. Shepard.*

Schirmerite . . . . . *F. M. Endlich.*
Schirmerite . . . . . *F. A. Genth.*
Schoharite . . . . . *A. Eaton.*
Schorlomite . . . . . *C. U. Shepard.*
Schreibersite (*m*) . . . . . *C. U. Shepard.*
Seybertite . . . . . *T. G. Clemson.*
Shepardite (= Schreibersite, *Shepard*) (*m*) . . . . . *W. Haidinger.*
Siderophyllite . . . . . *H. C. Lewis.*
Silicoborocalcite (= Howlite, *Dana*) . *H. How.*
Sillimanite . . . . . *G. T. Bowen.*
Sipylite . . . . . *J. W. Mallet.*
Somervillite . . . . . *A. Dufrênoy.*
Sonomaite . . . . . *E. Goldsmith.*
Spartaite . . . . . *A. Breithaupt.*
Spartalite (= Zincite) . . . . . *H. J. Brooke* and *W. H. Miller.*
Steeleite . . . . . *H. How.*
Stellite . . . . . *T. Thomson.*
Sterlingite . . . . . *J. P. Cooke.*
Sterlingite (= Zincite) . . . . . *F. Alger.*
Stibioferrite . . . . . *E. Goldsmith.*
Stirlingite (= Rœpperite, *Brush*) . . *A. Kenngott.*
Sussexite . . . . . *G. J. Brush.*

Talkeisenerz . . . . . *A. Breithaupt.*
Telaspyrine . . . . . *C. U. Shepard.*
Tellurbismuth . . . . . *D. M. Balch.*

Tephroite . . . . . *A. Breithaupt.*
Terenite . . . . . *E. Emmons.*
Texasite (= Emerald Nickel) . . . . *A. Kenngott.*
Thalite . . . . . *D. D. Owen.*
Thinolite . . . . . *C. King.*
Tincalconite . . . . . *C. U. Shepard.*
Torrelite . . . . . *T. Thomson.*
Trautwinite . . . . . *E. Goldsmith.*
Triploidite . . . . . *G. J. Brush* and *E. S. Dana.*
Troostite . . . . . *C. U. Shepard.*
Tungstic Ochre . . . . . *B. Silliman.*
Tysonite . . . . . *O. D. Allen* and *W. J. Comstock.*

Unionite . . . . . *B. Silliman, Jr.*
Uranothorite . . . . . *P. Collier.*

Vanuxemite . . . . . *C. U. Shepard.*
Venerite . . . . . *T. S. Hunt.*
Vermiculite . . . . . *T. H. Webb.*
Vermontite . . . . . *A. Breithaupt.*

Warwickite . . . . . *C. U. Shepard.*
Washingtonite . . . . . *C. U. Shepard.*
Wheelerite . . . . . *O. Loew.*
Whitneyite . . . . . *F. A. Genth.*
Willcoxite . . . . . *F. A. Genth.*
Williamsite . . . . . *C. U. Shepard.*
Wilsonite . . . . . *T. S. Hunt.*

Xanthitane . . . . . *C. U. Shepard.*
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Youngite (= Hornblende)

Zaratite (= Emerald Nickel) . . . . *A. Casares.*
Zincite (= red oxide of zinc, *A. Bruce*) *W. Haidinger.*
Zinc, silicious oxide, ... *L. Vanuxem* and *W. H. Keating.*
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CONTRIBUTIONS TO THE GEOLOGICAL HISTORY

OF THE

AMERICAN CONTINENT.

[The Address of the retiring President, delivered before the Montreal Meeting of the American Association for the Advancement of Science, 1857.[1]]

BY

JAMES HALL.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

ONE who accepts a trust has no right to make excuses for not fulfilling its obligations; and yet I would willingly excuse myself from the performance of this last act of the retiring President.

With imperative duties enough to occupy every hour of my time, I cannot do justice to you, gentlemen, or to your kind partiality in assigning to me this position. Circumstances beyond my control have placed it out of my power to make the preparation which was due to the occasion.

You have so often been reminded of the high objects and purposes which bring us annually together, that I need not repeat this now. It is the one great social and scientific festival of the man of science throughout the year, of all others, the most desired: — to the tired and lonely worker, who is endeavoring to add new truths to our stock of knowledge, or to elucidate more clearly what we but partially know; to the teacher of science who has

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special investigators in all these departments. Without their co-operation it would have been impossible to discover the relations of form and other physical characters with that fundamental arrangement of molecules whose nature it is now admitted controls all the properties of a substance.

The study of natural crystals has yielded rich material for the physicist. In the department of optics it has given results from which many fundamental laws have been deduced; and natural crystals, too, have furnished, in many cases, the very apparatus which made investigations possible. Some chemists claim that mineralogy is not at all a science by itself, and constitutes only a small part of inorganic chemistry. It can be unquestionably conceded that a knowledge of chemistry is fundamental, and in consequence this claim has a certain plausibility. On the other hand, we contend that it was largely the labors of the mineralogists on the physical characters of minerals, and especially their demonstration of the relation of form to chemical composition which finally awakened chemists to a more profound study of their own subject. The law of isomorphism was discovered by a chemist, whose training as an expert crystallographer in the examination of natural crystals made it possible for him to recognize the wonderful relation of form to composition. Dimorphism was first established from observations made on minerals, and it is in the study of the mineral kingdom that the laws of isomorphism and dimorphism find abundant demonstration. From the further investigation of the chemical nature of minerals we may hope for new light on the molecular constitution of substances which as yet the chemist has been unable to reproduce. We have already indicated the interdependence of geology and mineralogy. May we not claim the same interdependence of mineralogy, physics and chemistry, letting each go on in its own sphere, contributing to the general progress, sure that every new fact observed and every new law discovered will be for the common advancement of all?

APPENDIX.

A List[23] *of Names given to Minerals from the United States and Canada, for the most part described as New Species.*

Acadialite (= Acadiolite, *T. Thomson*)	*F. Alger* and *C. T. Jackson.*
Adamsite	*C. U. Shepard.*
Aglaite	*A. A. Julien.*
Alaskaite	*G. A. König.*
Albertite	*C. Robb.*
Algerite	*T. S. Hunt.*
Ambrosine	*C. U. Shepard.*
Amesite	*C. U. Shepard.*
Animikite	*H. Wurtz.*
Annite	*J. D. Dana.*
Anthophyllite, hydrous	*T. Thomson.*
Apatoid (*m*)[24]	*C. U. Shepard.*
Aquacreptite	*C. U. Shepard.*
Aragotite	*F. E. Durand.*
Arkansite	*C. U. Shepard.*
Baltimorite	*T. Thomson.*
Barnhardite	*F. A. Genth.*
Barytocelestite (= baryto sulphate of strontia	*T. Thomson.*
Beaumontite	*A. Lévy.*
Begeerite	*G. A. König.*
Bernhardinite	*J. M. Stillman.*
Bismuthaurite	*C. U. Shepard.*
Blackmorite	*A. C. Peale.*
Boltonite	*C. U. Shepard.*
Bowenite	*J. D. Dana.*
Brucite (= Native Magnesia, *Bruce*)	*F. S. Beudant.*

[23] This list is compiled almost entirely from the *Fifth edition of Dana's Mineralogy, and Appendices I-III to the same work*, in which will be found references as to where these minerals were originally described, as also any further facts published in regard to them, and their present position in the science.

[24] (*m*) signifies that the substance was found in a meteorite.

Tephroite . . . . . *A. Breithaupt.*
Terenite . . . . . *E. Emmons.*
Texasite (= Emerald Nickel) . . . . *A. Kenngott.*
Thalite . . . . . *D. D. Owen.*
Thinolite . . . . . *C. King.*
Tincalconite . . . . . *C. U. Shepard.*
Torrelite . . . . . *T. Thomson.*
Trautwinite . . . . . *E. Goldsmith.*
Triploidite . . . . . *G. J. Brush* and *E. S. Dana.*
Troostite . . . . . *C. U. Shepard.*
Tungstic Ochre . . . . . *B. Silliman.*
Tysonite . . . . . *O. D. Allen* and *W. J. Comstock.*

Unionite . . . . . *B. Silliman, Jr.*
Uranothorite . . . . . *P. Collier.*

Vanuxemite . . . . . *C. U. Shepard.*
Venerite . . . . . *T. S. Hunt.*
Vermiculite . . . . . *T. H. Webb.*
Vermontite . . . . . *A. Breithaupt.*

Warwickite . . . . . *C. U. Shepard.*
Washingtonite . . . . . *C. U. Shepard.*
Wheelerite . . . . . *O. Loew.*
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been freed from his daily routine of duties;— this occasion is one of many pleasant anticipations. We all meet as one brotherhood, as the children of one Parent under whose government we delight to live, and whose laws, impressed upon ourselves and upon everything around us, we would better understand. We come together for mutual counsel and mutual aid for the true interpretation of these laws respecting our relation to the animate and inanimate world around us, and our relations to the Great Author of the whole.

On every recurring occasion of our meeting it is a subject of congratulation; and more particularly for those who can so well remember when the year wore on without those pleasant anticipations of a reunion with sympathizing friends and colaborers in science. But this occasion, more than all others, gives cause for congratulation,— cause even to excite enthusiasm, that true to the enlightened views we profess, and true to the instincts of an unprejudiced scientific spirit, we have forgotten political and civil limits, have forgotten the names of governments and countries, and with cosmopolitan spirit we meet beneath the ægis of Her Britannic Majesty, Queen Victoria, though still upon the American Continent; and now may we more fully claim the right to the title of the AMERICAN Association for the Advancement of Science.

I might, were it the proper occasion, go on to say, in how far the establishment of telegraph wires, railroad lines and various social and commercial relations have tended to bring about this meeting in Canada; but scientific sympathies have done more; and I cannot help thinking too, that the discovery of another fact, that the rocks are of the same age and the same character in Canada as in the adjoining territory, has, in a manner served to assimilate the living inhabitants of this wide country, by showing most conclusively that the ancient inhabitants of this continent were of one nation and one government, and indeed are here today with us, if not to discuss, at least to furnish subjects of discussion, and who will maintain their ground notwithstanding geological systems and system makers. Nor should it be forgotten that the geologist of Canada, with that noble and unselfish spirit which characterizes the true man, and the man of real science, has adopted a nomenclature already framed for an adjoining State; for although directing the investigations over an area six times as large as New York, he saw nothing in the prolongations of the

same beds that could induce him to change the name of rocks already well determined. With this immense area Sir William Logan had a right to establish his own nomenclature; but in true scientific magnanimity, which affects no local nomenclature, Canada has been added to New York and the West, that over our common country may extend a unity of scientific language.

But from the present, which brings only delightful associations, shall we not turn one moment to the past, and to the memories of the departed? The last year has been a sad one for our little band of workers in science. Never has a year before cut down so many true and tried ones from amongst us. The names of Redfield, Tuomey, Mitchell and Bailey, need but be mentioned to find a mournful response in the heart of every one who knew them.[2]

. . . . . .

Our association, composed as it is of laborers in every department of science, has perhaps a right to expect that the retiring President should embrace in his discourse a view of the progress of science in America for the past year. But even if the investigator in one science were able to present such a view, I do not believe that the time allotted for our annual address is sufficient for such a retrospect.

For one who is in constant intercourse with the mute and inanimate existences of former creations, there is little hope that he can offer a language that shall be acceptable to the living, or that his subjects shall interest others than those who are themselves engaged in the same pursuits; and yet, gentlemen, in my poverty, I have nothing else to offer you than the results which flow from geological and palæontological investigations during the past few years, and to offer collaterally some *Contributions to the Geological History of the North American Continent.*

While I would claim for no science a preëminence, I must here protest against that narrow view which has been expressed by some, that Natural History is not a science, and who perhaps believe that mathematical formulæ lie at the basis of all sciences and that these must be used in all their minutiæ to reach any valuable result. But let us inquire whether, after all, natural science

[2] The long interval of time which has elapsed renders it undesirable to repeat here what immediately follows concluding the introductory portion of the address.

as it is termed by way of distinction (one might suppose to distinguish it from *artificial* science) be not truly a mathematical science. Every object in nature is a mathematical figure of some sort, and its form and proportions may be expressed in mathematical formulæ.

Let us take one of those little bodies known as crinoids, which are of various spheroidal or ovoid shapes. They are made up of plates of various forms, three, five or seven sided, with sides of various length and angles as various. More than this, these plates are not plane surfaces, and their curvature has such relation to the sides and angles, and to the form of the whole body, that when put together they shall produce just that form and no other. And now will you tell me by what processes a mathematician will set about calculating the length of the sides, the proportions of angles and the requisite curvature of each of these plates? Long before the mathematician could arrange his formulæ, the naturalist has instinctively measured all, and has reconstructed the body as unerringly as the motion of light.

But then you will ask me, perhaps, what is the use of all this? I will tell you of a use for it. When, like the navigator at sea, the naturalist may be wandering many hundreds of miles away from any fixed point in the geological horizon or geological sequence, he may meet with some dismembered plates of one of these bodies in the rock upon which he stands—no mathematical calculations, no astronomical observations or chemical analysis, will serve to determine the geological latitude and longitude. The density of the mass fails you, its chemical composition will not aid you, and you are thrown back upon the few poor pentagonal, hexagonal or heptagonal plates of a little insignificant body by which to determine the place. Do you measure the sides and angles and sit down to work out the formula with your known and your unknown quantities represented by initial characters? No! The naturalist instinctively, with mathematical exactness, sees the elements of the problem, and the result itself stands revealed before him. All that every other science has failed to do is here accomplished.

I might even occupy the entire time of this address in explaining the beautiful and curious structures of the crinoids. In some periods of geological history they are, so far as known, few, and their mode of construction is in each type very distinct, and every

plate is recognizable not only as referrible to the species, but to its place in the body.

In later periods of the palæozoic era there are great numbers of species constructed upon the same general plan, the same number and form of plates at the base, and in the first, second and third series or ranges of plates of the body; and although showing distinctions in the different species, they are not so strongly marked because of the great number of individuals constructed upon the same general plan. But as if in order to individualize themselves, and to overcome the want of character arising from similarity in basal structure, they have adopted another mode of distinction which is equally dependent on mathematical principles. The bodies are surrounded near the summit by arms, and there is a bilateral arrangement so that when viewed from one side which is called the oral side, the two halves correspond. Now on each side of this dividing line you will find the arms originating in one instance in pairs or double, and this prevails throughout, and you have ten arms in five pairs, *et cetera*. The first deviation from this is a group of *three* arms on each side of the ovarian aperture, while all the others are in pairs. Again you have groups of four on either side of the ovarian aperture, and groups of four in double pairs throughout the circumference. They present every possible combination of the numbers 2, 3, 4 and 5, in this manner individualizing themselves where the type is multiplied in species; and these are among the lowest forms which engage the attention of the palæontologist.

Without showing how, at every step from the lowest coral cell to the higher forms of life, the naturalist becomes the mathematician, it will be enough to mention to you the achievement of a naturalist (whose name I need not repeat here) in the conception and production of the outline of a fish from a single scale,—the solution of a problem upon purely mathematical principles aided by a profound knowledge of structural development.

Could the physicist have ever determined the original areas of land and water upon the face of the globe during former geologic periods, or have proved that the sea, once spread over continental areas, was always shallow during many successive creations?

Even when we had determined that our continent was outspread with the sediments of a former ocean, could any geometer show us the direction of the ancient current like our modern Gulf

Stream or its lines of greatest force and the lateral subsidence of this force? and could he point out the quiet mid-ocean areas that prevailed during that long, long palæozoic period, or during later geological ages?

Geology, then, may assume to know something of the structure of the earth, not less than the sciences which weigh and measure it; and when the vast accumulations of geological collections in our country shall be reduced to system, and the story of their origin told, we shall learn that there are laws in operation not differing from other natural laws, though seen and studied by different modes.

The picking up of stones by a child may not be science, nor is the blowing of soap bubbles by the same child any more a science, but either one of these amusements by a philosopher may lead to the discovery of laws which we call science.

It is here that one of the great difficulties of geological science lies; for geology presents herself to us in two aspects, the Physical and the Natural History. I might add also as a third, the Chemical aspect, so large a part has chemistry played in geological phenomena, being indeed at the foundation of many physical changes, and because there are no higher problems on which chemical science has expended her force than those furnished by geology.

To some minds, geology scarcely assumes the rank of a science, except where treated of from a physical point of view. They consider the simplest physical law adequate to the explanation of the most stupendous phenomena. To them, mountain chains rise and are abraded, and the entire crust of the earth is folded and plicated in obedience to certain laws. They see no difficulty in the way of imagining torrents of water moving onward and upward, carrying masses of rocks over heights far above their origin by some simple gyratory force. The entire earth becomes with equal ease to them, either a pliable, elastic, or compressible mass, or a non-elastic body; sometimes one, sometimes the other, changing gradually, or by sudden cataclysms, according to the fancy of the expounder. No wizard's wand ever played so many pranks as the poor earth in the hands of these theorizing geologists.

The more slowly moving, but not less industrious and devoted laborers in science, can never hope to climb the height of that Parnassus which the physical theorist in geology reaches at a single bound. We stand almost aghast at the passage and the

progress, but after all we are forced to confess that our *wonder* has been more excited than our reasoning powers; and I am in duty bound to say that geology has suffered more from these theories than from all other causes, though we have much to fear from the natural historians at the present time.

It is the misfortune of all sciences that when, having acquired a certain degree of popularity or interest in the public mind, men are found who, with more or less of true science, are still so fond of making theories of their own, that they at once invent a plausible and acceptable plan which goes forth to the world with all the circumstance attending a real scientific investigation. With truth enough to spare it from the anathemas of science, it still carries false doctrines and false views. Nor is this peculiar to geology, for other sciences have suffered equally from the false zeal of injudicious friends who attempt the reconciliation of new and legitimate views of science with old prejudices and preconceived opinions.

In geology this reconciliation is attempted between the science and Scripture, and every phase of geological science has had its reconciliation with the received opinions or prejudices regarding the origin of our earth, as derived from the first chapter of Genesis. The absurdity of all this is apparent, when we consider that with advancing discoveries every quarter of a century presents our science in so different a phase that a *new reconciliation* is required. Yet each reconciliation and interpretation is advanced with *ex cathedra* authority; and not satisfied with promulgating scientific truths, the author would insist upon making their interpretations dogmas of religious teaching and religious belief.

Another point which I conceive to be more pernicious than this is the Millerism in geology at the present time. But time will not permit a review of this subject.

Let me now, gentlemen, attempt to present you with something more peculiarly appropriate to this occasion; and, not to go beyond the pale of geological science, I may be permitted to call to your attention some of the more important results which have been derived from the investigations of the past few years.

There was a time when we were taught to believe that all the mountain regions of New England, the Appalachian chain, those of Northern New York and the Laurentian mountains, were pro-

jecting prominences of the ancient, irregular, primary nucleus of this portion of our planet; but that time has long since passed, and I conceive it to be one of the most important steps in the progress of modern geology that, having shown most conclusively that this science has nothing to do with the origin of things, we have proved that she recognizes no *beginning* of existing conditions. There is no *known* point that can be appealed to as a part of the primary nucleus of the earth, according to the theory which regards the earth as formerly an incandescent body.

All that we can see among the evidences of the earlier condition of the globe shows us that the physical conditions were then as now: that even the *beginning* which we recognize in geology shows our continent as an ocean-bed laved by the widespread waters, and solid materials broken down and transported then as now by currents and spread out in stratified deposits over the ocean-bed. The evidences of these conditions meet us in the elevated ranges of the Adirondack mountains in New York and the Laurentian mountains of Canada, and various isolated points elsewhere. These most ancient of all the rocks which we know tell us only of *water* and its distributing action in first spreading abroad the materials, and their subsequent metamorphism and elevation.

How many similar sedimentary formations may have preceded this one, we know not, but we have reason to believe that this is not the earliest, and we may yet know more of those of prior date. And even now the investigations in the Canadian Survey have shown the necessity of dividing the sediments of this period which we have considered as one; the facts proving these rocks to have been formed during two epochs, forming the Lower and Upper Laurentian.

We have moreover the evidence of preëxisting stratified deposits, for here in these ancient sediments of the Laurentian period are found enclosed masses of preëxisting stratified rocks. Even these ancient crystalline masses, which had been believed to be the oldest if not the very primordial nucleus of the earth, are found to contain laminated masses, not conforming to the stratification or lamination of these beds, but lying at a different angle, telling you that here is a fragment of some *older* stratified mass, which had been broken off, transported by the ocean current, and embedded among the finer débris of a later deposit. Even here then we find

that not only are these rocks not the primary nucleus, but that the materials of which they are made are not derived directly from that nucleus.

Instead therefore of the earlier condition of our planet exhibiting the action of great heat, and the results of igneous influences, we find only evidences of water, for metamorphism does not necessarily imply the action of heat in a high degree. Here are the oldest rocks known, still preserving their lines of bedding and their outcrops, as distinctly marked as in the more modern formations.

We are indebted to the Geological Survey of Canada for fully establishing this *Laurentian System* as a series of older stratified deposits and the promulgation of another, the *Huronian System*, which must hereafter form a part of the geological history of our continent.

These Laurentian deposits of immense thickness, and regularly stratified, are nevertheless almost throughout highly crystalline. In the gneiss of this period, which preserves its regular lamination, were embedded large masses of a preëxisting gneiss, with its planes of lamination lying at various angles to the lines of bedding in the enclosing rock. Masses of this kind often appear like an immense accumulation of bowlders, the interstices of which have been filled with sand and mud, the whole having become indurated and subsequently metamorphosed; the bowlders retaining the character and lamination of their original bedding, while the new deposit has assumed its own system of lamination conformable to the lines of bedding.

In this manner we have disposed of these areas of crystalline rocks belonging to the more ancient class, or those prior to any evidence we have of the appearance of life upon our planet; but the analogy in their mode of occurrence, and consequently their manner of formation with the fossiliferous deposits, induce the hope, and I may say even the expectation, that we shall one day prove them to have been the seat of animal and vegetable life. Notwithstanding that our experience thus far negatives such inference, I hold that there is nothing unphilosophical in the belief that all the conditions of their deposition, so far as we now can see, were as favorable to life as the newer formations, and there is no ground upon which we can safely conclude that animal remains will not yet be found in these rocks. That there was a period

when life began upon our earth I do not doubt, but there are no evidences beyond those of a negative character, to prove that it might not have begun long previous to the commencement of those rocks at present known as fossiliferous. This same negative evidence has been applied to New England and other portions of the country long after many of us had shown that the mountains of New England consisted of strata of fossiliferous age; and it will probably be a long time yet before all will admit that the Green mountains are Silurian, and the White mountains Devonian and Carboniferous, in their age.

In one respect the lower crystalline rocks present a point of high interest. In their greatest development they are known only towards the north, both in America and Europe. On this continent they rise to the northeast, forming a long barrier nearly parallel to the line of the St. Lawrence; and thence stretching away to the westward by the great lakes they gradually die out in low isolated areas, which form no strong feature, and are finally entirely obscured by later formations. It may still be said that we have intrusive granites of older date, and that these may be parts of that original nucleus. There is no evidence to sustain this view. Granitic masses penetrating the Laurentian rocks may still be portions of the preëxisting stratified deposits; and when we find crystalline granite intruded in the more recent Silurian, Devonian or Carboniferous strata, we may infer that they are derived from the formation entirely beneath. We have no evidence that, even by granitic veins or extruded masses of so-called igneous granite, there is any communication from the surface with that imagined primary incandescent nucleus of our earth.

This point prepares us for a different appreciation of the phenomena presented by crystalline masses. If it does not simplify the whole subject of geological science, it at least takes away the source whence have been derived a large part of those absurd speculations, where imaginary laws of physics and chemistry have been applied to illustrate and sustain an equally false and imaginary geology.

The grand problem of geology is the entire history, chemical, physical, zoölogical and botanical, of the groups of strata constituting the formations of the globe. From the first evidences of life as we know it in the strata, to the present time, is the phase in the earth's history with which geology has most to do; and it

is here that the labor of systematizing and classifying has been mainly expended. But even with the best intentions and with the best efforts there are different views expressed, and one type or another, as seems most in accordance with the author's views or his knowledge, is made to mark the eras or periods. This is true not only of the more general but also of the more detailed classifications which have been advanced and adopted.

All these classifications must, confessedly, be founded upon imperfect knowledge, for the geology of the globe has been but partially studied, and negative knowledge, or the absence of all knowledge, is too often treated in generalizations as positive knowledge.

It might be presumptuous to hope to advance any new views upon this subject; still there is one which seems to me not likely to receive sufficient attention in the increasing interest and attractions of palæontology. This science instead of being studied in connection with geology, and as an exponent of geological phenomena, has come to be regarded as a great object of independent investigation; and rocks are looked upon merely as the conservatory or museum of the natural history of the past geological periods. Certainly I ought not to oppose a view which exalts palæontological science; and still, gentlemen, I would not in this study lose sight of the attendant physical conditions, which are equally important to be regarded. The progressive steps in the development of animal life have not been made without great physical changes; and we can never arrive at a true history of our planet till both are studied in connection.

The relation of the physical conditions to the vital force and to its development is a problem of vast importance. It should always be borne in mind, that the expression of animal life is coincident with physical conditions, and that the vital force is never developed independent of physical conditions. This is true without giving any undue importance to these conditions, or any countenance to materialistic views.

The subdivisions into formations, groups and systems, have always been marked by physical changes. Where a group of strata has its materials like those of the preceding group, there will the forms most resemble those of the preceding period; and where the lithological change is greatest, there will be found the greatest change in the nature of the organisms. When an intervening

formation cuts off all the species of a preceding group, and the next, or third, is similar to the first in lithological character, so far will the fossils resemble each other not only in generic forms, but in the characteristics of species. We have then the converse of this proposition, that uniform physical conditions give uniform fauna. And I may here say, that while this holds in a large majority of instances, it does not hold true in all; because there is a law still beyond that of physical condition which affects the geographical distribution of species. In modern zoölogy, we have geographical centres of distribution; and we have zoölogical districts or areas both on land and in the sea. Here questions are involved which have not been heretofore fully discussed; but which nevertheless — so long as each geological period is marked by a fauna of its own, and is compared with a modern fauna — demand our serious consideration.

In pursuing this subject, let us for a moment review the recognized periods of American geology, or the geological systems and groups as at present accepted, and their application to the rocks as developed upon our continent. With true scientific spirit, as well as in respect to what has been done on the continent of Europe and in Great Britain, we endeavor to make the greater groups or systems harmonize with those adopted in Europe, while all the minor divisions must be wrought out and receive the expression due to them from the conditions of this continent, dependent upon the prior conditions of the great ocean in which they were deposited. In tracing the development and distribution in that part of our continent which we intimately know, I shall be able to show with more force the results at which I have arrived respecting certain *mountain* and *continental phenomena*.

The Potsdam sandstone, in its wide distribution, is everywhere marked by ripple marks, by broken and drifted shells of *Lingula*, diagonal lamination and all the evidences of a shallow sea. In the uniformity of the character of its materials and its even distribution over wide areas, it is only equalled by its associated member the Calciferous sandstone or Magnesian limestone which, in composition and the uniformity of physical conditions under which it was deposited, presents similar characters. Even long prior to the period of these depositions, the forces which had disturbed the preëxisting continent had ceased, and in contradiction to the generally entertained opinion that in more ancient times

there was more disturbance of the crust, there has scarcely ever been, since this advent of life upon our globe, so wide an area or so long a period, indicating repose of all the disturbing forces. At no period has deposition been so uniformly diffused or animal life so widely distributed in the same forms over so large a proportion of our continent.

The second great period of the geological history is marked by the limestone groups, the Chazy, Birdseye, Black River and Trenton. The latter is the most important and gives character to the whole. This formation we know to be widely spread over the continent, indicating until near its close a period of repose, and it is only in some few points towards the northwest that we discover the deposit much attenuated and the influx of muddy sediments interfering in a great degree with the persistence of the fauna.

I might state here incidentally that the fauna of the Potsdam and Calciferous period is totally unlike that of the Trenton limestone period following. In the earliest of known periods when life has flourished, it has been widely distributed and has ceased without convulsions or cataclysms, leaving the area of ocean which it had occupied for the occupancy of new races. In the very first of our creations, therefore, we have evidence to show that life was terminated over wide areas while there are no evidences remaining of violent commotions.

The next succeeding or the Hudson River group appears to us with more features of interest in its physical characters than either of the preceding; and it was at this period, as I shall attempt to show, that the form and outline of our present continent were determined.

The Hudson River group presents us on the one hand with a series of soft shales becoming coarser and alternating with sandstones above, and on the other with irregular masses of limestone and finally immense masses of coarse sandstones or conglomerates with great bands of shale. This group attains in Canada a thickness of 2000 feet, and in Pennsylvania it has a thickness, according to Prof. Rogers, of 6000 feet. Its northeastern extremity stretches out to Gaspé, and on the south side of the St. Lawrence, and, throughout Canada and the Green mountains of Vermont, forms in its metamorphic and folded condition, a great feature of the country. In its continuation southward it forms a marked feature in the Appalachian chain gradually dying out as the range declines

to the southward. Along this line, which I shall term its line of trend, it presents no striking variations—nothing but what might be expected in any sedimentary group of rocks. It is only in pursuing it to the westward that we find interesting and important changes. Tracing it through Canada, west by the islands of Lake Huron and by Green Bay to the Mississippi River and more southerly still by Cincinnati and the mouth of the Ohio, we learn that the coarser sediments gradually disappear, the finer mud alone having been transported, and finally that calcareous matter becomes an important ingredient in the formation and sandstone has become an exception. Or to give the best expression to the change, we here in New York and Canada speak of the shales and sandstones of the Hudson River group terminated by the Oneida or Shawangunk conglomerate. In Ohio the same group is known as the "Blue limestone." In Wisconsin and Illinois, this group, which in New York and the east is thousands of feet in thickness, has thinned so that it measures only from sixty to one hundred feet, while in Iowa I have not been able to obtain a thickness of fifty feet. Here we have an extensive group of strata changing from fine and coarse sedimentary materials and passing through all the intermediate phases until it becomes a shaly limestone.

Now in looking at the present continent as at that time an open sea, I conceive that the most rational explanation of these phenomena is that a powerful current has brought these sedimentary materials of sand, clay and pebbles from the northeast[3] and has distributed them along the line now marked by the Appalachian chain; or, if you please to assume that there has been a continent to the east of our own which has been subsequently submerged, then the materials have been distributed along its coast. On whatever ground we view the deposit, we must admit, I conceive, that these materials have been distributed by a current, and that the direction of this current has been in the line of greatest accumulation and coarser materials; and that towards the westward, where we find less accumulation and finer materials, but still a wide ocean, the current gradually diminished until it essentially ceased and the fine materials were slowly spread over the broad area which they occupy in their diminished thickness. Such I conceive to have been the physical conditions of this an-

[3] See note at the end of address.

cient ocean-bed, an ocean disturbed by currents as extensive and as powerful as any we can point to in modern oceans. Thus from an equable and comparatively quiet period when the Potsdam sandstone was spread widely over the bottom of the sea, we have come to one when powerful currents directed the distribution of the sediments in long lines of deposition, on the seaward side of which we find the gradual thinning-out of the beds for want of a sufficient transporting agency.

Shall we here inquire, what influence this change in the character of sediments has had upon the fauna of the period; and if the fauna in any manner indicates the force of physical influences? In the east the Hudson River group presents an abrupt change from limestones to black and green shales containing few fossils. In the sandstones above and in the calcareous bands accompanying them are numerous plants, graptolites, some crinoids and acephalons, with gasteropods and cephalopods not known in the group below; while at the same time after being absent through the lower part of the group, many of the Trenton limestone species of brachiopods are here reintroduced midway in the series mingled with the new fauna.

In the west where the strata are calcareous and have a lithological similarity with the formation below we find, likewise, a greater similarity with the fossils of the preceding period. Nearly all the Trenton brachiopoda are found, together with a large proportion of new forms of this family. The acephalous shells, which are the distinguishing fossils of this group in New York and part of Canada, are rare and very subordinate in the west, forming indeed no notable portion of the fauna. At the same time the trilobites of the Trenton limestone occur abundantly in the Hudson River group of the west while they are extremely rare in its coarser sedimentary phase in the east. But even these phases do not hold good throughout, and in western Wisconsin, northern Illinois, and in Iowa towards the attenuated margins of the group, scarcely a single species of the group is identical either with those of Ohio or of New York or Canada; and, finally, on its attenuated northwestern margin it is marked by scarcely more than half a dozen species of fossils, not one of which could be recognized as identical with anything known in the group within two hundred miles from the centre of its greatest development. Such are the phases of

the Hudson River group in the great eastern development and in its western and southwestern prolongations, and this was the extent of our knowledge of its phases until within a year past [1856]. During this time the investigations of the Canadian survey under the direction of Sir W. E. Logan have extended to the Island of Anticosti, hitherto almost a *terra incognita*, and the few fossils we had seen from there indicating very clearly a new and rich field. But the results are far in advance of all anticipation.

In order, however, to understand fully these results and the position of that Island, you must know that from this point to below Quebec and still farther down the St. Lawrence, the lower formations with the Hudson River group in its vastly developed physical aspect, together with the formations below, abut against the slopes of the Laurentian mountains; that to the eastward the trend of the Hudson River in its conglomerates, etc., is more easterly while the Laurentians trending to the northeast open a space between them in which lie the Island of Anticosti and the Mingan Islands. We might have expected to find on the Island of Anticosti the upper sedimentary deposits of the Hudson River group with some higher beds, but we were quite unprepared for the result which shows that here, as at the west, are large accumulations of calcareous beds with the recurrence of fossils similar to those along the area exposed by the Cincinnati axis. The same corals, the same brachiopods that mark the "blue limestone" formation of Ohio and Indiana and of the islands of Lake Huron are, here in Anticosti, characteristic forms. On the west the nearest points where these associations of fossils are known are the islands of Lake Huron, three hundred miles or more to the westward of where we stand; while on the other hand, seven hundred miles or more to the eastward, are the same associations of fossils. In Anticosti there is the well-marked coral *Favistella stellata* with *Orthis subquadrata*, marking the same horizon as one thousand miles farther to the west, while all the intermediate space is filled with the sedimentary deposits of the group, in which neither of these forms occurs. To the southward of Anticosti, there has been the great force of that current from the northeast bringing forward the sediments of the Hudson River group, while the more gentle action spread northward the finer material, to be mingled with the calcareous matter there formed from corals and shells and to become

the habitation of another fauna. And not only do we find there many species which we know as characteristic of western localities, but many other similar forms both of corals, brachiopoda and gasteropoda, besides many new and remarkable forms. Instead therefore of this group of brachiopoda and other fossils forming, as we had supposed, a peculiar fauna marking the western extension of the Hudson River group, it has, in truth, its centre farther to the east, and the western apparition is an isolated offshoot, as it were, which after all has had but a meagre development when compared with that of Anticosti. The trend of the coarser materials, striking against the southern prolongation of the Laurentian range, cuts off the western extension of these more quiet sea-deposits which were only again reproduced and continued at the westward and southwestward.

The strata which we designate therefore as our Lower Silurian formations have been deposited under very unequal conditions: in the first instance an almost universal sediment of uniform character, ending with calcareous materials marked by a fauna as peculiar as any other; then an entire cessation of these conditions and a new fauna scarcely less universal in its distribution in the earlier epochs such as the Trenton limestone. In the later phases of the Hudson River group it exhibits evidence of violent physical action in the accumulation of coarser debris, powerful currents and unequal distribution of material, with a varied fauna. Certain parts of the group in its geographical distribution are characterized by fossils which are almost or quite unknown in other parts.

Almost coincident with the distribution of the sediments of the Hudson River group to the west and southwest are those of the Medina sandstone and Clinton groups. These later formations gradually die out in the line of the Great Lakes on the westward and have scarcely a vestige in the Ohio valley; while they are, like the Hudson River group, much more persistent in the direction of the Appalachian chain. Although we can only regard these products as the result of the same action which produced the sediments of the Hudson River group, yet we admit the culmination of that action and of that group in the Oneida conglomerate; while the Medina sandstone and Clinton groups in their sedimentary materials must be regarded as the result of the diminishing force which originated and continued in greater or less degree

throughout the period. At the time the Medina sandstone (and perhaps the Oneida conglomerate) as well as the coarse materials of the Clinton group were being deposited in New York and westward, the sediments of Anticosti were made in a comparatively quiet sea, and the succession of forms goes on almost without interruption. You perceive that there were already at this early period vastly greater accumulations of sediment along what is now the eastern border of our continent than along the country to the westward and especially in that part now constituting the Mississippi valley.

We are now to contemplate a return to a more quiescent condition of the ancient ocean. The shales, sandstones and conglomerates are succeeded by limestone formations of wide extent. The Niagara group, stretching from the Hudson valley westward, can be traced through New York, Canada, by lake Huron and lake Michigan, thence across Wisconsin and Illinois, where it disappears beneath the Devonian and Carboniferous rocks of Iowa and Missouri. In its southwesterly direction from New York it gradually thins out, but is still known in Tennessee where it is lost beneath the more recent formations. The numerous fossils in the Canada Survey collections proclaim the existence of this group in the peninsula of Gaspé and at numerous other points on the northeast.

Without at this moment giving the deductions which arise from the facts, I may mention here that the Niagara group and succeeding limestones lie uncomformably upon the Hudson River group, in the valley of the Hudson; that, even before the deposition of this member of the series, those contractions which have finally produced the results of which we have been before informed, in the Green Mountain and Appalachian chain generally, had at this early period commenced.

The Medina sandstone and Clinton groups, which lie at the base of the Upper Silurian System and below the Niagara group, have had but a partial distribution of sediments in a westerly direction, while the succeeding limestones of the Niagara are as widely distributed as any Silurian formation upon our continent. Notwithstanding this disturbed condition during a large part of the time of the deposition of the formations at the base of what we term Upper Silurian, a new fauna was introduced, and as we should sup-

pose under most unfavorable circumstances. Short periods of repose indicated by the nature of the strata, alternated with periods of disturbance from the influx of coarse sediments, which must have been disastrous to animal life. We might have presumed that the fauna we find in these rocks was a colony, as it were, from some more favorable centre, but we have not until now had the means of proving that in the same period the island of Anticosti was a quiet sea and there flourished the prototypes of our upper Silurian fauna.

The conditions which existed during the Niagara period, caused by the elevation of the eastern portion of the Hudson River group, thus producing a thinning of the easterly edge of the Niagara group, continued after the cessation of its fauna. Beginning with its thin edge in eastern New York it becomes conspicuous in western New York and Canada West and is thence recognized by the islands around the northern margins of the Great Lakes and becomes an important formation at the Mississippi River. Thus without any apparent disturbance and with a continuance of the same physical conditions which continued to cause the deposition of the same kind of sediments, we have a total change in the fauna marking this later stage, as it were, of the Niagara period. The conditions for its production becoming more and more favorable as we go westward to the Mississippi valley, and there, in its greatest mass and the highest development of its fauna, we leave it passing beneath the more recent formations of Iowa with a thickness of six hundred feet.

In the succeeding order, the Onondaga Salt group follows, parallel in its outcrop and so far as we know coëxtensive with the Niagara group through New York, Canada West and thence to the Mississippi valley; and again southwesterly parallel with the Appalachian chain. Although having this great geographical extent, it has generally a moderate thickness and in some places has not received the recognition of a distinct group. Generally the deposit is that of a shallow sea, while in New York and Canada West it presents the features of a deep basin deposit. While thinning out on the eastern limits of New York to a thickness of thirty feet or even less, and westerly to less than one hundred feet, it still has in central New York a thickness of 1000 or 1200 feet; showing that there was a deep depression, caused in all probability by the lateral

pressure on the east, which elevated the rim of the basin on that side rather abruptly while it sloped more gradually to the west. This lateral pressure on the east is apparently caused by the gradual contraction which produced the more abrupt foldings of the crust in the region of New England and eastern New York, thus showing most conclusively that changes of level and modifications of the form of the ocean were going on throughout these periods and that librations extended much beyond the actual evidences of their action at this time.

I have chosen to speak of these formations where we know them fully and have trusted to no doubtful observations or evidence not fully sustained by facts. You will have perceived that thus far the Silurian sea has spread over all the known area of the older rocks on this side of the Rocky mountains. In the period of the Potsdam sandstone and lower limestones the depth was very uniform throughout, as would appear from the fauna. In the period of the Hudson River group we trace the deposits, as we believe, to greater depths than animal life of a molluscan type can flourish, while further to the east were shallower seas. In the Niagara period and the two succeeding ones, the ocean presents a pretty uniform character over a westerly extension of one thousand miles from one meridian, while we have evidence of libration on the east and particularly in that portion occupied by the sedimentary portions of the Hudson River group.

The lines or exposed areas of deposit mark the ancient ocean limits. When the conditions were favorable for sustaining life we look to the fossils for the indications of the changes of area and depth of this ancient ocean. In the absence of life, we seek an explanation of these changes of area and depth, in the direction and force of currents as indicated by the nature and extent of the sedimentary deposits.

If we now look to the next succeeding group of strata, which we term the Lower Helderberg group, we find the direction and extent of its area almost at right angles with those immediately preceding it, and parallel to that great line of sedimentary deposits marking the crest of the Hudson River group. From the investigations of the Canada Geological Survey, the Lower Helderberg group is known on the northeast at the extremity of Gaspé, resting unconformably upon the rocks below; thence to the south-

west more or less interruptedly it pursues its course, here at our feet in the bed of the St. Lawrence appearing in outliers which rest unconformably upon the upturned edges of the lower beds of the Hudson River group. Again in the Hudson valley above the Highlands lies the same series resting upon the shales and shaly sandstones of the Hudson River group, while the continuation of that group to the northward in its high crest of the Shawangunk is cut off by the erosion of the Hudson valley. Tracing the group but a little way westward, it passes beneath the Upper Helderberg in apparent conformity to the nearly horizontal Hudson River rocks below and those which succeed it. Southward we trace this group of strata through Pennsylvania, Maryland and Virginia to Tennessee, where its horizon is well marked. But notwithstanding this immense extent, from Gaspé to Tennessee it rarely extends more than a hundred miles to the westward of the Hudson valley in New York though having a greater width to the southwest. It therefore presents a belt of fossiliferous beds of about fifteen hundred miles in length from northeast to southwest. Here then we have the entire area of the west and northwest, as far as we know (with the exception of a very small area in Illinois and Missouri), entirely destitute of this formation or of any representative of it in the series. Was this vast area a deep sea or dry land? If a deep sea, we should expect to find the fine sediments of the shoreward formations drifted out into it to some extent; but these we nowhere find. In a few instances we have seen the lower beds apparently water-worn and broadly furrowed as if exposed to shore currents or even to littoral action, but of these conditions we have too little knowledge to speculate. But it still appears to me that we must admit the conclusion that at this period a large proportion of the area on the northwest of the line I have indicated must have been above water.

Thus while the trend of the limestones of the Niagara and the two succeeding calcareous groups has a general east and west direction, this limestone of the Lower Helderberg has a northeast and southwest trend. While the Lower Helderberg limestones by their fossil relations are Upper Silurian and intimately related to those below, the trend of greatest accumulation of the two is nearly at right angles.

The line of deposit of the Oriskany sandstone and Caudagalli

grit was coincident with that of the preceding limestone deposit and overlies it. The sandstone, indeed, stretched somewhat farther to the west and lay beyond the edges of the limestone upon the upper members of the Onondaga salt group; still the same area is occupied, and the line that admitted of the formation of coral-reefs in the Lower Helderberg period has become again, as in the period of the Hudson river group, the line of currents and of sedimentary deposits. This sandstone formation which in Gaspé is more than one thousand feet thick, and in Pennsylvania according to Professor Rogers, seven hundred feet thick, soon thins out to the westward. In the Helderberg mountains it has a thickness of not more than thirty feet, and often less than three feet, while farther west in central New York it occurs in a few isolated patches of a few feet in extent and less thickness, filling some small depressions in the upper beds of the Onondaga salt group. Here we have apparently again returned to the conditions of northeast and southwest accumulation of sediments deposited in the line of the Appalachian chain.

The Oriskany sandstone is the period in which acknowledged types of Devonian fossils are introduced. It lies parallel to the preceding Upper Silurian limestones and at right angles to the great mass of the succeeding fossiliferous Devonian beds. These types which were introduced in the period of a sedimentary rock which was deposited along a line from northeast to southwest, and cut off from the succeeding group by a non-fossiliferous belt (the Caudagalli grit), nevertheless appear and flourish in the next formation whose trend is nearly at right angles to the preceding, and the vital manifestations go on irrespective of the change in the physical conditions.

Succeeding the period which I have just described, where the sediments have a northeast and southwest direction, we have to contemplate a calcareous formation whose known trend along the line of outcrop is nearly east and west, stretching from the Hudson valley through New York and Canada West, thence across Michigan, Indiana and Illinois, to the Mississippi river and into Iowa, having an outcrop parallel and apparently conformable in position to the rocks of the Niagara, Galt [Guelph] and Onondaga salt group. The belt of formations just noticed as the Lower Helderberg limestone, the Oriskany sandstone and Caudagalli grit is

an important one along the eastern outcrop, but in the west these formations are entirely absent, and what is more remarkable, their absence is not noticeable, and for all we can determine on the ground, the sequence is as perfect in the west as at any point in our palæozoic area.

The calcareous beds before alluded to, including the Schoharie grit, constitute the Upper Helderberg group. The Schoharie grit has very little western extension, but is known for at least two hundred miles in a southwest direction. The limestones of this group mark a line of terrace from the Hudson river westward throughout New York, and present one of the best marked and most extensive physical features of the entire state. It is true the attenuate easterly margin of this group may be traced from the Hudson valley to Tennessee, and the same is true of the Niagara and Onondaga salt groups, though the line of greatest force is from east to west, and the whole series presents but a meagre thickness in Tennessee.

Pursuing our investigations southward from this limestone terrace, we find in eastern New York and in Pennsylvania, first: a series of shales and shaly sandstones; then we have a series of sandstones and shaly beds; and above these, shales and sandstones all of which may with little difficulty be included in one group as has been done in the geological survey of the latter state. But in central and western New York we find the lower part of the series becoming more shaly, and finally more calcareous, so that in the western half of the state it is a group of calcareous shales with beds of limestone known as the Hamilton group, and containing a set of fossils peculiar to itself.

The central portion of the mass becomes better defined as a series of shales and flagstones, with some heavy beds of sandstone which are well marked, though nearly destitute of fossils. This group of strata has received the name of the Portage group.

At the same time the third member has become defined as a set of shales and shaly sandstones with a peculiar assemblage of fossils, and has received the name of Chemung group.

When we follow these successive groups of strata to the westward we find them gradually thinning out. The Hamilton group traced through Canada West, Michigan and Illinois to the Mississippi, there presents a thickness of some fifty or sixty feet, while

in eastern New York it is scarcely less than fifteen hundred feet, and is a thousand feet thick in the central part of the state. The Portage group, which in its defined limits in central and western New York has a thickness of one thousand feet or more, is scarcely represented in the Mississippi valley.

The Chemung group, to which in New York I would not attribute a thickness of less than twelve or fifteen hundred feet, is often represented in the Mississippi valley by some thirty to fifty feet, and sometimes by one or two hundred feet of shaly sandstone and beds of limestone. These formations together in Pennsylvania have a thickness, according to Professor Rogers, of five thousand feet to the southwest.

Now regarding the formations in this aspect only, we should conclude that here were transverse currents carrying materials from the east to the west, and this conclusion was reached before we knew anything of the structure of the New England mountain series, and before we understood as we now do the geology of the northeast. The researches of Sir William E. Logan on the peninsula of Gaspé have shown that the *equivalents*, and in fact the *continuation* of these groups, as a part of the Devonian system, have an enormous development in an unaltered state, and in the condition of coarse sediments of at least seven thousand feet in thickness and with a southwesterly trend.

If we follow the line of trend we are taken along the direction of the White mountains, thence into Massachusetts and New York, where we are taught that higher metamorphic sediments overlie authentic Lower Silurian, until we find that the unaltered and undisturbed Hamilton, Portage and Chemung groups of New York are but the prolongation of the Gaspé sediments which have become folded and metamorphosed in the region of the White mountains, but which reappear on the west in their unaltered condition and highly fossiliferous character as soon as out of reach of this disorganizing action of metamorphism.

Instead, therefore, of finding here any *new* force or diverse transporting currents, we have simply a return of the great transporting agencies of the Hudson river period, from the same source apparently in the northeast, and prolonged in the same direction to the southwest, parallel and coëxtensive with the Appalachian chain. The finer materials were spread laterally

through the more quiet ocean, till they nearly disappear by thinning out in the Mississippi valley. You will see, gentlemen, with what an enormous amount of sedimentary deposits I am loading the eastern side of the future incipient continent, an amount too great perhaps for credibility, but I have not yet done. From the same source and in the same direction has come the vast amount of sediment forming the Catskill mountain group, with the capping conglomerate, having a thickness of more than three thousand feet in New York and six thousand in Pennsylvania, while the group has entirely thinned out before reaching the western limits of the state, and no vestige is known of it in the Mississippi valley.

Beyond this are, according to Professor Rogers, three thousand feet of red shale at the base of the coal measures and extending parallel to the Appalachian chain. The origin of this shale has not been satisfactorily traced in the northeast, but there is no difficulty in finding a source for this and other sedimentary deposits of the Coal Measures, and an evidence of the same transporting force in the fourteen thousand feet of Lower Coal Measures found by Sir William E. Logan to exist in New Brunswick.

Now, gentlemen, I am satisfied I have wearied your patience too much in these details, but I could not present a simple conclusion without first giving you the data in full.

I have shown you incidentally, as we progressed, that the Hudson river group became disturbed, folded and upturned before the deposition of the Niagara, and still more before the deposition of the Lower Helderberg group which has its trend parallel to this great belt of formation, and which lies upon the upturned edges of the Hudson river group, from Gaspé to the Highlands of the Hudson.

Indeed, I believe the origin of the coarse materials of the Medina and Clinton groups may be due to disturbances among the preëxisting beds of the Hudson river group, and probably derived from the breaking up of the conglomerates of this group in its northern prolongation. Sir William E. Logan has likewise proved that with the commencement of the foldings and plications of these lower strata commenced the metamorphism of the same. And here I cannot forbear from alluding for a moment to the views of Mr. Hunt

upon metamorphism. Laying aside all assumption of causes not known to exist, and ignoring the supposed effects of that imaginary nucleus, he has proceeded, from known and demonstrated facts derived from existing conditions, to explain most satisfactorily certain processes in metamorphism; and, instead of appealing to an unknown source for the ingredients of certain metamorphic strata, he has demonstrated the existence of the same elements in the unaltered beds which are known to be of the same age and prolongations of the same strata.

The system of folding and plication therefore, which has been attributed to later violent action, has been going on from the commencement of the Upper Silurian, and though its later phases include the Coal Measures, it has nevertheless been produced by continued action from a long anterior period. Nor has it ceased with the coal, but I believe has been continued in a less degree since the deposition of the sandstone of the Connecticut valley. But even for one moment admitting the supposition that these foldings and contortions have been for the greater part produced at a single period, let us inquire whether this has had anything to do with the production of the mountain chain, or whether this system of mountain-making by upheaval, overturning, etc., is a system of mountain-making, and whether rhetorical eloquence has not made us believe the language without testing the facts. I must assert that it has nothing at all to do with it—that so far at least as regards our mountain chains, the principle in its application is false.

You have seen how, age after age, by the simplest of all processes, the immense sedimentary deposits have been transported and distributed along the line of our great eastern mountain chains; and with the accumulation of sediments this line was depressed time after time, as new deposits were piled upon it; and now at the end of the Devonian period, or at the end of the Coal period, has the whole continent been raised to its present elevation. If there were no folding or contortion, and if the strata of the Alleghany mountains were as undisturbed as those of the great plateau of the west, what would have been the result? Simply that, instead of the Appalachian mountains of six or seven thousand feet in height, you would have had a simple stratified pile of more than twenty thousand feet from the base of the Potsdam to the summit of the Devonian.

But have folding and partial upheaval along mountain chains nothing to do with the grand elevation? I reply, nothing at all; but, on the contrary, every one of these disturbances lessens the effect and degrades the elevation. It is *original deposition* that not only gives direction to our mountain chains, but amount of deposition that determines their elevation; or, if you will reduce it to the simplest terms, we may state that *elevation is due to deposition*, and there can be no great degree of elevation without a corresponding amount of previous deposition of sediments.

Now I conceive this to be the simplest system of mountain-making that can be proposed, and it can be shown to be applicable not only to the Appalachian chain, but to other mountain ranges of our continent.

Let us see, moreover, whether the same simple principle, somewhat extended, may not be applied to continent-making. We admit that the Appalachian chain has given mainly the eastern outline to our continent, and that original deposition has given origin to this mountain chain. The currents of the palæozoic ocean have determined the directions of these sediments, and consequently of the mountain ranges and the outlines of the continents.

A series of strata similarly deposited by an ocean current, but nearly at right angles, mark the western outline of our continent; and though modified by subsequent depositions, they are nevertheless fundamentally of like origin, due to the same causes, giving origin to a mountain chain and the outline of a continent.

Now, if we extend our observation to the Mississippi valley, a thousand miles to the west, where we have essentially the same set of strata as those forming the Appalachians, we find no mountain ranges. I shall be told that it is because there are no upheavals, but this is not known to be true. Several axes and extensive faults cross the valley of the Mississippi nearly at right angles to the Appalachian chain, elevating all the strata from Lower Silurian to Devonian. Nevertheless, though the strata are elevated from ten to thirty degrees, they produce scarcely a perceptible feature in the prairie country.

Uplifting has not produced elevation, but has rendered the strata liable to degradation, by breaking or weakening them along the line of fracture or folding. I have already shown that the groups of sedimentary strata composing the Appalachian chain have thinned

out to a few hundred feet at most on the Mississippi and there are consequently *no materials* of which to make mountains in the great Mississippi plateau. The general height of the country gives the entire elevation produced by the thickness of the beds, and there can be no more. We could have no mountain chains along a great plateau like this, however much the strata may be folded and contorted, unless there should take place a dislocation of the older Laurentian or Huronian rocks from below; and although such a phenomenon would be in accordance with geologic *theories*, respecting the upheaval of mountain chains, yet I suspect that careful search would prove that no such upheaved chain of mountains exists upon the surface of the globe. In fact, that the upheaval of mountain chains, except in the sense that the elevation of a continent is upheaved, has never taken place. In this connection let me offer an example in point in support of what I have said. It has been stated that the White mountains are composed of accumulations of sedimentary deposits of Silurian and Devonian age, having altogether in their undisturbed condition, a thickness of from fifteen to thirty or forty thousand feet (or more, if I follow authority), while at the same time the White mountain chain little exceeds six thousand and the Green mountains not more than four thousand feet in height. Now in tracing this mountain chain to the southward, we observe, on the west side of the Hudson river above the Highlands (which are produced by an area of older rocks), the Catskill mountains reaching an elevation of about four thousand feet, and stretching away to the westward in gradually declining isolated hills. These have been regarded as part of the Appalachian chain but they have nothing to do with it so far as being involved in the disturbance of the strata. They stand alone as an isolated group, of which the nearly horizontal pile of strata gives this great elevation. The disturbed Hudson River group makes one or two hundred feet of their base. *Deposition* has here given *elevation* without loss or degradation by any of those foldings and plications which are supposed to lie at the foundation of mountain-making. Had the strata of the Green mountains and White mountains remained undisturbed, and in the original condition of deposition, they would have overtopped the Catskills by several thousand feet.

Original deposition, then, directed by ancient currents, has un-

questionably determined the direction and elevation of our mountain chains.

The time on this occasion will not allow me to extend the application of the same principle in unravelling and explaining complexities, that may often occur—when,during the depression of a continent or of a portion of it which may afterwards form a mountain chain, transverse or diagonal currents may cause such an amount of deposition as to produce a great complexity of structure, when the continent shall become elevated.

The simplicity of the North American continent is due to the originating causes producing the direction of the palæozoic currents; while a large part of the eastern side has not been subsequently submerged, to any great degree, and has thus escaped the causes of complexity. The wide extent of later deposits in the interior of our continent, which give contour to that portion, was in a great degree paralleled with the eastern deposits. The forces which we say have rent the eastern, the central and the western, are merely the vast accumulations of material which become elevated, perhaps simply from the great accumulation, thus rendering the escape of heat from the interior slow or impossible. And in this statement, it must not be forgotten, that the central portion of our continent, and some parts of the western portion, were not continental, but submerged areas, till after the Cretaceous period, and to some extent also, as late as the Tertiary period; and if these have greater elevation it is due to greater deposition.

Although it is not necessary to introduce the Coal Measure deposits into the Appalachian system for the purposes of explaining their elevation, they are nevertheless complicated with it, in a great degree. But all the phenomena, so far as regards folding and plication, and the production of the northern mountains as well as a large part of the Appalachian system, do not include the Coal Measures in the elevation. On this account I would call your attention to some facts connected with the deposition of the coal formation in the United States. But before proceeding to do so, I must dwell for a moment on the Carboniferous limestones, or those great calcareous formations which, in the valley of the Mississippi and the southwest, lie beneath the Coal Measures.

In tracing the origin of our sedimentary formations from the northeast, we do not find any cessation from the beginning of that

period called Devonian till we come to the Coal Measures. The whole country in this direction would seem to have been submerged, and the course of these ancient currents to have continued with little interruption.

In the Hamilton, Portage and Chemung groups, we first find *land plants* and these remains increase towards the northeast, while they are rarely or never known in these formations to the west of the limits of New York. In Canada, however, similar plants are known, and the account of a specimen in the collections of the Survey, published by Mr. Dawson, shows that dry land may have been near, and we have every reason to believe that dry land capable of producing vegetation did first appear in that direction and in the early Devonian period. While in the east, the Catskill mountain rocks, and the red shale beneath the anthracite Coal Measures, were being deposited, we have in the west no representatives of those rocks; the Carboniferous limestones beginning their deposition directly upon the Chemung group.

The condition of the ocean in the west during these periods is worthy of attention. Upon the cessation of the sediments of the Chemung group, commences a limestone formation of great extent, and marked by numerous species of fossils. By a well-defined line, this lower limestone is separated from a succeeding one, marked by a wholly distinct set of fossils; while its greatest development lies to the south of the area of greatest development of the first. In the same manner a third, less strongly defined; and with an intermediate sandstone formation, still another limestone succeeds, and this with its northern limit far below or to the south of the first and second, and its greatest development is to the southward of the preceding members.

Now the simple explanation of this seems to be that the sea, in which limestone deposits could be made, was contracting on the north and gradually extending to the south and southwest, while we have evidence to prove that much dry land existed to the north,—evidence which I shall presently attempt to show. This fact of the gradual expansion of the ocean to the southwest, and its contraction on the north, is significant when taken in connection with the succeeding formation of the Coal Measures; for which reason I introduce here the notice of these facts.

It might appear unnecessary while so much has been written,

and so well written, upon the subject of the Coal Measures of the United States, to offer anything in this place, but I believe I can present one or two points of interest in this connection.

I have already called your attention to the fact, that the sedimentary materials of the coal formation have their greatest expansion to the northeast, and that we find the earliest record of land plants in that direction. The Coal Measures, as a whole, though widely expanded, show a gradual thinning-out to the west and southwest, and in Iowa and Missouri have become greatly attenuated as compared with the same formations in Pennsylvania, Ohio and Kentucky.

I have just now reverted to the fact that on the north and west, we have evidence of dry land, or of land raised above the sea previous to the deposition of the coal. While in Pennsylvania, Ohio, etc., the Coal Measures apparently rest conformably upon the strata below them, in Iowa and Missouri they rest unconformably upon the rocks below. While the prevailing dip of the older rocks is to the southward, there are nevertheless numerous faults and axes which uplift the strata in a direction from N. W. to S. E. or at right angles to the Appalachian chain.

In the west these ancient rocks of Lower and Upper Silurian and of Devonian age have, previous to the deposition of the coal, presented numerous inequalities of surface, partially due to the broken strata produced by elevations and faults, and partially by subsequent denuding action.

In Illinois, Iowa and Missouri we find the Coal Measures spread horizontally over the uplifted edges of these older strata, sometimes barely filling the depressions and inequalities of original surface, giving origin to numerous little circumscribed basins, often of only a few yards in extent. And not only do we have the evidence of ancient worn and unequal surfaces of this ancient land, but we find the sand mud, and carbonaceous matter often filling fissures, and penetrating into ancient caverns, which must have been worn while these older strata were near to or above the level of the sea. The wearing down of the crests of low axes, and the broken margins of faults, prove the existence of a powerful eroding action, and as we know that at this period the sea was gradually retreating to the southward and the land rising on the north, the motion of the abraded materials would be in the same direction.

There are other phenomena connected with the coal period, which are of much interest. In Pennsylvania and Ohio, we observe several intercalated bands of calcareous shale or argillaceous limestone, charged with marine fossils, thus indicating that the area of the Coal Measures was sometimes depressed beneath the sea. These bands of limestone with marine fossils are known in the Coal Measures of Ohio, Illinois, Iowa and Missouri. The limestone of this period in Missouri has been usually referred to the Carboniferous limestone, not recognizing these beds as distinct from those below the coal; and it was not until the publication of the Missouri Report that Professor Swallow clearly indicated the position of the beds on the Missouri River as a part of the true Coal Measures.

In the meantime and for many successive years, fossils of Carboniferous age had been found in the far west along the Rocky Mountain range from as far north as Fort Laramie and the Great Salt lake, by Capt. Stansbury, and at numerous intermediate points by the Pacific Railroad Surveys and other United States explorations, and on the Southern Boundary line and in New Mexico by the Boundary Commission. Dr. Roemer also had discovered some fossils from the Carboniferous limestone in Texas. We had therefore a knowledge of the vast extent of a limestone along the Rocky Mountain range from latitude 43° north, to about 30° south, containing fossils referred to Carboniferous age, and the rock was almost by common consent referred to the age of the true carboniferous limestone and has been cited as Lower Carboniferous limestone.

Having had occasion during the two preceding years to give some attention to the rocks of Carboniferous age, and more particularly during the past year to a comparison of the fossils, I was surprised to find that some of these southwestern species were identical with those from the well-developed carboniferous limestone in the Mississippi valley, Iowa, Illinois and Missouri to Alabama. Furthermore a careful comparison of species collected by myself many years since from the limestones of the Ohio coal measures, with others collected by Mr. Nicollet, Capt. Stansbury and by Messrs. Meek and Hayden from the limestones of the Missouri river, showed a most positive identity of species. A farther comparison of all these with the carboniferous fossils from

some of the Pacific Railroad Surveys and those of the Boundary Survey proved such positive identity of species as left no doubt that all the limestones of the southwest, that is, in the approaches to the Rocky Mountains and beyond them, are of the age of the coal measures. A later comparison of the collections of Marietta College has enabled me to demonstrate the identity of several of the species described by Dr. Hildreth with those of the southwest, and it may be unnecessary to add that these are Coal Measure fossils.

These limestones, then, of the southwest, spreading over an area many times as large as all the Coal Measures of the remainder of the United States, are the greater development of those thin bands of limestone with marine fossils that mark certain lines in the Coal Measures of Ohio, etc. These bands of limestone, from insignificant beds of a few feet in thickness, have extended to several thousand feet in thickness and mark great lines of mountain ranges, and, lying often nearly horizontal or dipping at various low angles, present vast cliffs along mountain passes towering thousands of feet above the plain below. And these, gentlemen, are limestones of the Coal Measures, while the productive Coal Measures are unknown over this vast region. With what propriety, then, can we say that the coal period is preëminently the period of vegetation, as is done to sustain a theory that it marks one day in the creation of the world, or to sustain another view that coal vegetation gives the character to the entire palæozoic series?

I regret, gentlemen, that I cannot bring you some very brilliant theoretical result from all these data; but the very simple explanation of all is, as I have shown you step by step, that the northeasterly portion of our continent was either already dry land, or was just rising from beneath the water at this period; that as low ground just emerging from the ocean, and at intervals deluged with sand and mud and lying little above the water level, it sustained a luxuriant growth of vegetation; while the gradually retreating ocean still spread over by far the largest proportion of the continent on the west and southwest. The accumulated reefs of limestone, three thousand feet in thickness, attest the long period of quiet, and freedom from irruptions of sedimentary matter upon these regions. At intervals the unstable position of the newly made dry land permitted the incursions of this southwestern ocean, in a shallow ex-

pansion over the great Coal Measure area, only to return again, upon the gradual elevation of the surface, and thus once more to become a low land fitted for the production of plants.

This subject followed out promises us some of the most interesting and beautiful results, and will show us how the librations of the surface have been connected with the destruction and reproduction of the successive floras of the coal period.

There are numerous interesting generalizations arising legitimately from the facts I have brought before you, but of which time will not permit a notice. In no direction, however, more intimately than in that of palæontology, do these facts and physical deductions point to interesting and important conclusions. There is one fact, which in this connection I cannot omit, in regard to the fossils in these intercalated beds of limestone in the Coal Measures, and this is—that they are species of the same genera and many of them of allied forms to those characteristic of the limestones below the Coal Measures, or of the Devonian and Silurian limestones; showing conclusively that the condition of the ocean did not essentially vary through this long period, and the only difference between the coal period and that of the preceding was that there was a larger area of dry land giving origin to the great growth of vegetation of that period. Moreover, the large area of this ocean formation, characterized by a marine fauna of the same period, compared with the relatively small extent of the areas of carbonaceous matter and terrestrial vegetation, shows that so far from being essential, they are rather incidental conditions of the Carboniferous period.

CONCLUSION.

In the progress of the earth from the beginning of animal life to the present time, we are but watching the progress of an ocean bed to its final development as a continent,—and for the most part, every step has been one of progress towards the present condition. Whenever any portion of this ocean bed has been raised above the surrounding sea, land plants have taken possession of that portion. If it has remained long enough above the surface, land animals have become its denizens. As a matter of course, the higher land animals did not first appear, for even now, simi-

larly situated lands do not sustain the higher organizations. The occurrence of mammalia in the Jurassic period is no more irrelevant to that period, than the occurrence of plants in the coal period. It is a fact perfectly in accordance with all the rest of our legitimate information, that the organic conditions have always been coincident with the physical conditions.

Geology, if we would let alone grand theorizing, is a simple and beautiful study, in which we see everything evolved naturally and harmoniously, without at any time great and sudden changes. We remark those changes as one who having viewed a city in its progress, should fall asleep for a century and afterwards behold the difference. But to one who could have seen stone laid upon stone, and each edifice completed singly, it would have had but the aspect of natural and quiet progress.

NOTES.

PAGE 37. The gneiss cited as Laurentian and containing bowlders of preëxisting gneiss I have seen in the region of Lake Superior, and they were referred to the Laurentian age on the authority of Sir W. E. Logan. I am informed by Dr. T. Sterry Hunt that these masses are of Huronian age and carry boulders of stratified Laurentian gneiss. Although this shows that we have carried back the stratified gneiss only to the Laurentian period, it proves nothing in favor of a granitic nucleus nor of extruded granites of igneous origin.

PAGE 41. A comparatively quiet condition, during a large portion of the period of its accumulation, is indicated by the uniform fine-grained character of the quartzose sand, which extends over large areas. The tracks of Crustaceans, found by Sir William E. Logan upon certain surfaces of this rock in Canada, appear to have been at about the same level along the strike of the formation for 400 miles. The deposit was evidently made in shallow water, and from the mark of a wind-ripple found upon the surface in connection with these tracks at one locality, it is evident that the sand had been laid bare at the ebb of tide.

The formation in Canada has a thickness of from 70 to 600 feet, and an extent of a thousand miles flanking the Laurentides from Bellisle to its extreme southwestern extension in the Province. "The hills, at no very great distance from the outcrop of the Potsdam formation, rise to heights varying from 500 to 4000 feet; and while the sand at their base lay between the ebb and flood tide, the flank of the Laurentian mountains must have formed the coast of the Lower Silurian sea. As has already been stated, these hills extend from Labrador to the Arctic Ocean, and we can thus trace out the ancient limit of the ocean for 3500 miles."

This formation extends southward through New York, Pennsylvania, Virginia, and notably through Tennessee, and occurs flanking the older metamorphic rocks in Texas. The formation is extensively developed in the States bordering the upper Mississippi and in the upper Lake Region. It likewise constitutes an important feature in the Geology of the Black Hills country.

The character and thickness of the formation in New York, along the

eastern flank of the Adirondacks, is similar to that existing in Canada. In the Mohawk Valley at the southern extremity of the Laurentian gneiss it is often scarcely recognizable, and never attains more than a few feet in thickness in any of the exposures. In Pennsylvania the maximum thickness of the formation is from 2500 to 3000 feet, while in some localities its thickness does not exceed twenty-five feet: — this variation being due, in some degree at least, to the original inequalities of the sea bottom.

In Tennessee, according to Professor Safford, the Potsdam group has a thickness of more than 20,000 feet; but this includes the conglomerates below and the magnesian limestones above.

The source of the Sandstone flanking the Adirondacks has been attributed to the subjacent Laurentian gneiss, and it has been suggested that long continued trituration has broken up and separated the mica, hornblende, etc., which have been floated away, leaving only the siliceous grains to form the great deposit. It may, however, be questioned whether the gneiss has been the chief source of material. In the great conglomerate or breccia (for the fragments are mostly angular), at the base of the Potsdam on the southeastern flank of the Adirondacks, the fragments of gneiss are few and small, and the greenish material, which may have been derived from the mica and hornblende, is in small proportion. By far the largest constituent of the mass is of quartzite, corresponding with a similar material found in many places lying upon the gneiss, and sometimes referred to the Potsdam, while in some localities it appears to have more intimate relations with the gneiss below.

Fragments of feldspar are sometimes found embedded in this conglomerate, and, in the more interior localities, a larger proportion of fragments is evidently derived from the underlying gneiss rock. In Tennessee, Professor Safford represents the lower portions as "coarse gray conglomerates, talcose, chlorite and clay slates, repeatedly interstratified, all having a semi-metamorphic aspect." But it should be recollected that the adjacent metamorphic rocks may be of newer age than the Laurentian. Again "the pebbles are quartz and feldspar mainly, generally rounded, but sometimes more or less angular." The conglomerate "usually contains more or less chloritic and talcose matter in its interstices."

Along lines of the most active erosion, no large deposits are made, and those which are made are of a coarse character; the great accumulation is just outside of the limits of the most active forces.

The underlying conglomerate or breccia is widely disseminated, having a greater thickness at the south (in Tennessee) than in the north. This may be accounted for by the gradual subsidence of the entire continent, or the eastern portion thereof. The deposition of sediments apparently began at the south, coincident with the subsidence, and there was a recurrence of similar conditions during the whole period of depression, so that at the base we have a conglomerate which is followed by a sandstone; and in Tennessee, where the subsidence was greatest and farthest removed from the final source of sediments, we have the greatest thickness, and the uppermost beds are shales, indicating deep water and distant source of material.

In the original nomenclature the term Potsdam sandstone included conglomerates and quartzites resting on the Laurentian gneiss, with the sandstones above these, to the base of the calciferous sandstone (magnesian limestone). It has been found, however, in some localities, that there is a gradual passage from the Potsdam to the calciferous; and above the latter a repetition of the previous conditions in the deposition of a fine grained sandstone in the northwest known as the St. Peter's sandstone; while in Missouri there are several alternations of sandstone and magnesian limestone, showing that similar conditions were recurrent to the close of the series.

It seems pretty clear, therefore, that while the magnesian limestone was being deposited in mid-ocean there may have been a deposition of siliceous

sand along the coast line or littoral zone, giving a continuous series of sandstone layers, or, as sometimes occurs, with thin calcareous interlaminations. We can conceive it possible that the Potsdam fauna may have lingered along the shore lines, even after the close of the deposition of the magnesian limestone.

In Tennessee, Professor Safford includes all these in the Potsdam group, recognizing three divisions; the lower division of conglomerates and slates; the middle, the Chilhowe sandstone, with intercalated beds of dolomite; the upper including the Knox shale and the Knox dolomite which closes the series. The whole constitutes a series of deposits most naturally grouped together, as belonging to a single geological period, and they are represented as conformable throughout, and are equally involved in the later movements which have folded and plicated the mass as shown in the mountain exposures. The series, wherever of considerable thickness and exposure, is susceptible of subdivision, and Dr. Owen suggested a triple division of the mass below the magnesian limestone in the upper Mississippi Valley, while a different subdivision is applicable to Missouri. Thus far no one has successfully attempted the correlation of the subdivisions recognized in different parts of the country.

The relations of the fossiliferous beds at Troy, N.Y., referred to the lower Potsdam, have never been satisfactorily determined. It is a matter of mere conjecture. The fossils are from calcareous masses embedded in slates whose age and relations are unknown and which in fact cannot now be separated from those of the adjacent Hudson River group. Therefore it is unsafe to attempt to correlate, with these beds, strata in other portions of the country.

(3) PAGE 42. The northeastern source of the mechanical sediments has been questioned by several writers. The greater thickness of many of the formations in Pennsylvania, than to the northward, has induced Professor Lesley to infer that the source of sediments has been in the southeast, instead of the northeast as I have suggested. By northeast I would be understood to mean northeast of the centre of the Appalachian chain. The rapid attenuation of all the sedimentary deposits above the Potsdam, to the south of Maryland, and the increasing proportions of the calcareous formations in the same direction, seem to me to indicate that the great source of sedimentary deposits was to the north and northeast. In the southwestern continuation of the Hudson River group we find it passing gradually into calcareous material. Taking the entire series from the base of the Hamilton to the top of the Catskill, including the upper member or Pocono sandstone, embracing altogether a thickness of more than 10,000 feet, we find the whole represented in Tennessee by a few hundred feet of strata. At the north the strata of the Hudson River, Medina, Clinton, Hamilton, Portage and Chemung groups are largely arenaceous; while in the south the corresponding formations except the upper ones are mostly calcareous. Had the source of the sediments been from the south, we should have to assume that the coarse deposits were made at a great distance from this source and that they were transported across a large area which now preserves only the evidence of gentle forces and open sea accumulation. It would seem, therefore, that this proposition is neither necessary nor satisfactory, and that the source of sediments was probably from the north or northeast.

PAGES 44, 45. In 1857 the Canadian Survey had recognized the entire belt of slates, sandstones and conglomerates on the southeast side of the St. Lawrence, including also a considerable proportion of the metamorphic rocks farther to the eastward, as of the Hudson River age, and the subject was thus treated in the address.* Subsequent investigations proved a large proportion of this area to be of older formation, but notwithstanding this

* Prof. Rogers, and other geologists, had before this time recognized the metamorphic rocks of New England as of palæozoic age.

fact, the reasoning regarding Anticosti and its relations still remains correct. The island is a mass of strata in which calcareous matter prevails to the exclusion of coarser sediments.

Owing to the more northerly trend of the Laurentides from the Pt. de Monts, a wide space to the northeast has been occupied by the ancient palæozoic ocean in which were deposited the several members of the series from the calciferous to the Niagara inclusive, in quiet succession.

The intermediate space between Anticosti and the Mingan Islands on the north has been occupied by the upper part of the Birdseye and Black River limestones, and by the Trenton limestone proper, the Utica slate and lower part of the Hudson River group; the upper members of the latter, to nearly 1000 feet in thickness, forming the northern shore of Anticosti. This gives to the lower formations, from their base to the top of the Hudson River group, a thickness of more than 2600 feet. A large part of the island, however, is composed of the succeeding formations. The Anticosti group consists of representatives of the Medina, Clinton and Niagara groups, but these are everywhere calcareous, having a maximum thickness of nearly 1400 feet.

The sequence of open sea deposits of this region, from the Trenton to the Niagara groups, has, therefore, apparently been more complete and less interrupted than at any other point within our knowledge. Unfortunately, the hiatus between the Mingan Islands and Anticosti leaves us without positive knowledge of the nature of the deposits in this interval. The coarser sediments of the Hudson River group, in its typical localities, occur in the upper part of the series, and since here we have the higher portion of the group represented by nearly 1000 feet of calcareous beds, it is fair to infer that it may have been calcareous throughout. In its physical aspect as well as in its fossil contents, the Hudson River group at Anticosti is comparable with the series of the same age in Ohio and Indiana, the northern part of Kentucky and in Tennessee.

Crossing from Anticosti to the peninsula on the south side of the St. Lawrence, we find the Hudson River group represented in its finer and coarser sedimentary deposits as known in its typical localities, and here as well as at Montmorenci near Quebec and elsewhere, complicated with the older slates, sandstones, etc., of the Quebec group. From Gaspé, therefore, by the south side of the St. Lawrence to Quebec, and thence to Montreal and by Lake Champlain and the Valley of the Hudson to the Highlands of New York and through New Jersey, and Pennsylvania to Virginia, we have this belt of sedimentary strata, usually more or less disturbed and complicated, and sometimes partially metamorphosed, but always well-marked and clearly distinguishable over an extent of outcrop of nearly 1500 miles.

PAGE 46, ¶¶ 3, 4. Of late the question has been raised regarding the unconformability of the Niagara group and subsequent formations, to the rocks of the Hudson River group below, and consequently the date of the foldings and plications of the latter have been controverted by geological writers. The original idea of unconformability was maintained by Rogers and Mather of the N. Y. Geological Survey while Emmons in one case explains the apparent want of conformity as due to faulting, while he gives another section showing conformability. Sir W. E. Logan everywhere maintained the nonconformability of these formations. I have held this view from my own investigations, though expressing some doubt in a single instance, but I have no longer any hesitation in my own mind regarding the matter, for wherever there may be *apparent conformability* it can only be delusive.

At St. Helen's island near Montreal, and elsewhere, the limestones of the Lower Helderberg rest unconformably upon the inclined and eroded edges of the Utica slate or other lower members of the Hudson River group, with an intervening conglomerate in which are found pebbles of Trenton limestone, Utica and Lorraine shales. The entire superficies

of the valley had been eroded previous to the deposition of this Lower Helderberg limestone. Numerous localities along the Hudson River valley present evidences of want of conformity between beds of the Hudson River group and those of the succeeding formations. Those which have been chiefly cited are Rondout and Catskill or between these two places on the west side of the Hudson, and Becraft's mountain on the east side. The more conspicuous sections are near Rondout, but one of the most interesting occurs at Austin's Mill near Catskill. At this locality the general aspect of the section presents an appearance of conformability between the formations below and those above. A critical examination, of the line of contact, however, presents not only distinct discordance of the beds, but in one place the line of junction is marked by a thin stratum of grit with rounded pebbles separating the sandstone of the Hudson River group from some calcareous layers of the age of the Niagara group. This point is in the trough of a synclinal and the lines of stratification are essentially parallel. At another place in the same exposure near the edge of the synclinal are presented the upturned edges of the Hudson River sandstones and shales, succeeded by a thin band of grit and breccia; and upon this rests a soft magnesian limestone of the age of the Onondaga Salt group, succeeded by the haydraulic limestone and members of the Lower Helderberg series.

The apparent conformity in many cases is an illusion; for, although the successive beds may appear to be in accordance, so far as bedding is concerned, a more careful examination may show that the strike of the one crosses that of the other, as is seen in the exposure at Austin's Mill, where the divergence of strike between the Hudson River strata and superimposed formations is at least thirty degrees at one point on the edge of the synclinal.

It should be noted that in the greater number of junctions described, between the Lower Helderberg and Hudson River groups, the limestones of the former group are often found resting on the shales and slates which characterize the middle and lower members of the Hudson River series, while the upper arenaceous beds of the group are wanting. Nor would absolute contact and parallelism of beds in this case prove conformity of structure. It is quite impossible that complete conformity should exist between the beds of the Hudson River group and any portion of the Niagara or Onondaga group, in the absence of the entire series of the Medina sandstone marls and the shales of the Clinton group; and when it is proposed to bring the members of the Lower Helderberg into parallelism with the Hudson River group, the absence of several thousand feet from the base of Medina to the top of the Salt group shows the impossibility of such conformity. Unfortunately, the absolute junction of the two formations has rarely been carefully sought — where a few inches of conglomerate or breccia may represent the interval between the close of the Hudson River and the beginning of the Helderberg period — an interval of thousands of feet in thickness and ages in time.

The beginning of the disturbance was at the close of the Hudson River period and the great thickness and extent of the red marls of the Medina indicate that the deposit was not made in an open sea. A vast interval separates the Lower Helderberg from the Niagara. The whole Onondaga period with its marls and dolomites of one or two thousand feet in thickness is interposed. The open sea was shut off and a region with salt lakes spread over the formations of the preceding ocean area for a thousand miles to the westward (as now evidenced in its salt and gypsum beds), while thinning out on the east. Over this eastern outcrop and far eastward swept the Lower Helderberg Sea, which laid down the limestones in the valley of the St. Lawrence, on the flanks of the Beloeil mountains, east of Montreal and from Gaspé* by Maine, New Hampshire and Vermont, and from New York to Tennessee. The break, shown by all these phenomena,

* The limestone in Gaspé is 2000 feet thick.

ending in the unconformable spread of Lower Helderberg, alike over the filled up Onondaga basins and uncovered Niagara and Hudson River groups in eastern New York; over eroded Utica slate in eastern Canada, and over older crystalline rocks from New England to Gaspé, marks the first great geological revolution from Potsdam time.

PAGES 49–51. The term *trend* of the formations was used to indicate, as far as known, the line of greatest accumulation or the line of transport of the material. Since this knowledge is derived from the outcrops, and much intermediate ground remains unexplored beneath superincumbent formations, it has been urged that the inferences may not always be technically correct; but I do not conceive it probable that any erroneous conclusions of importance are likely to result from such statements of the fact, —at least the actual outcrops furnish more reliable information than any system of hypothesis.

The Oriskany sandstone, from Gaspé to Virginia in its trend or line of greatest deposition, is essentially parallel to the Lower Helderberg. In New York it extends, as a marked formation, farther west than the Lower Helderberg, and is also found in Canada West. Both the Oriskany sandstone and Lower Helderberg group are known in southwestern Illinois. The eastern extension of the Oriskany is overlaid by the Caudagalli and Schoharie grits. In central and western New York these formations are absent, and the Oriskany was exposed to eroding influences during the time of their deposition, and until the beginning of the deposition of the Corniferous limestone. Through the central and western part of New York the Oriskany sandstone is found filling scattered depressions of limited extent in the preceding formations. Sometimes the lower beds of the Upper Helderberg group contain rounded and angular fragments of this sandstone, showing conclusively that there was considerable erosion of the strata below, previous to the formation of the limestone.

The sandstone is unknown (except as small isolated patches or nodules, or as fragments embedded in the limestone above) between Seneca Lake, in central New York, and the Niagara River, and yet near Cayuga, in Ontario, after an interval of 200 miles, there are outliers of the rock yielding an abundance of the characteristic fossils of the formation. It is, therefore, fairly inferred that the absence of this rock in many places may be due to erosion, while the remaining evidences of its existence might readily be overlooked.

PAGE 55. The Address has been facetiously criticised as proposing a system of mountain making with the mountains left out. The Address was not intended to propose any system of mountain making, but to show that mountain ranges were coincident with lines of great sedimentary accumulation. That this accumulation of sediments with its subsidence and consequent folding and plication, and the subsequent elevation of the mass and erosion of the anticlinals, had shaped the mountains; that no mountain elevations could take place where the sediments composing the area were thin; and that the mountain elevations were never equal to the vertical thickness of the strata composing them. I intended to imply that mountain elevation was due to sedimentary accumulation and subsequent continental elevation — trusting to the intelligence of my hearers and readers to interpret my suggestions.

From various sources giving the thickness, I stated that the maximum of the palæozoic sediments, entering into the formation of the Appalachian chain, was 40,000 feet. Perhaps it would be more prudent, as a basis of argument, to accept a medium and place the thickness at 25,000 feet (though the aggregate is much greater), out of which have come mountains of 5000 feet in height. It may not be easy to account for the manner in which the enormous erosion has been accomplished, for this could not have taken place beneath the sea, and the most natural explanation is that the eastern part of the continent has at some subsequent time been greatly elevated to allow of such erosion. I did not pretend to offer any new

theory of elevation, nor to propound any principle as involved beyond what had been suggested by Babbage and Herschel. I did not propose to discuss the theory of the contraction of the globe from cooling, or of the crumpling of the earth's crust from the gradual cooling and shrinking of the interior mass, because such arguments are not always philosophical for want of a basis in facts, and are always unsatisfactory as giving a very inadequate solution of the problem. This question cannot be properly discussed in a note.

I am satisfied that a region where the ocean bed, during palæozoic time, subsided so as to permit a deposition, under water, of more than 25,000 feet of strata, is sufficiently unstable in character to come up again to an elevation required for the erosion of the anticlinal valleys. This great subsidence alone may indicate that the area was one of weakness and liable to elevation or depression according to the action of the forces.

During the long palæozoic time the area of subsidence was in the Appalachian region, though clearly enough, during some portion of that time great uplifting occurred on the northeast, to be succeeded by subsidence which may have been equal to the elevation. Why could not the area of subsidence be changed from the Appalachian region to the ocean on the east? Subsidence in one locality means a corresponding, but not necessarily equal, elevation elsewhere; so while the ocean bed was subsiding may not the Appalachians have risen?

PAGE 57. See the introduction to Palæontology of New York, Vol. III.

PAGE 59. I have shown the existence of a similar condition in Iowa, where the erosion of the Devonian and Silurian surfaces has been covered by coal measures, preserving little evidence of the eroded material. In many instances of this kind deep depressions of considerable area were worn in the older formations, which were filled up by the underclays of the coal measures and often followed by the deposition of the coal above. (See Geological Report of Iowa, pp. 120–141, 1858.)

PAGE 61. Very recently Mr. Gilbert of the U. S. Geological Survey has discovered coal and coal measures in the far west, just enough to make an exception to the general condition here stated.

NOTES ON PROF. JAMES HALL'S ADDRESS.

BY T. STERRY HUNT, LL. D., F. R. S.

The present publication, after more than a quarter of a century has elapsed, of Prof. James Hall's Address before the American Association at the Montreal Meeting in 1857, is eminently fitting at the present time and the views therein expressed will be found for the most part to have lost none of their value, but to have been confirmed by the progress of geological science. It should here be mentioned that most of the important points in this Address were published by Prof. Hall in 1859, in the Introduction to the Third Volume of his great work on the Palæontology of New York, and were the subject of an extended notice by the present writer, in an Essay on Some Points in American Geology, in the American Journal of Science for May, 1861. It was here shown that the views of Prof. Hall, which when heard and read in 1857 and 1859 seemed to many untenable and evoked hostile criticism, were in harmony with those previously maintained by masters in the science of Geology. It is true that the elevation-hypothesis of Von Buch had been extended from volcanic mountains to ranges made up of aqueous sediments, like the Alps, the Pyrenees and the Alleghanies, and by the authority of Humboldt and Elie de Beaumont had been impressed upon the scientific world, but not without protest. Constant Prevost, Scrope and Lyell had opposed this famous hypothesis as applied to volcanic elevations, while de Montlosier in 1832, in the same spirit, declared that the great mountain chains of Europe

are but the remains of continental elevations which have been cut away by denudation and that the foldings and inversions to be met with in the structure of mountains are to be looked upon only as local and accidental.

J. P. Lesley, again, in 1856, had asserted that continents are but congeries of mountains, or rather the latter are but fragments of continents, separated by valleys which represent the absence or removal of mountain-land; and again had said "mountains terminate where the rocks thin out."

Later, the present writer returned to the discussion of Prof. Hall's views in an Essay on Some Points in Dynamical Geology published in the American Journal of Science for April, 1873, and in part a criticism of Professor Joseph LeConte's paper which appeared in the same Journal in November and December, 1872, on the Formation of the Features of the Earth's Crust. I quote from my Essay the following passages, to show how far Hall's views were accepted by LeConte, and also for the reason that in reply to the adverse criticisms of LeConte and others, I have here stated what I conceive to be the great and distinguishing merits of Professor Hall's contribution to the theory of mountains first developed in his remarkable Address of 1857.

"The views of Professor Hall as to the relation between great accumulations of strata and mountain elevations, are cited with approval by LeConte, who, following him, asserts that 'mountain chains are masses of immensely thick sediments.' I venture, however, to remark in this connection that the views both of Mr. Hall, and of myself, as his expounder, have as yet been but imperfectly understood either by LeConte or our other critics. Thus, they have been defined as a theory of mountains with the origin of mountains left out, while LeConte says 'Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain-formation has taken place.' Now in fact neither Hall nor yet myself in my exposition of his views [in the Essay of 1861 cited above] has proposed any theory to explain this part of the process; that is to say, the uplifting of the deposited sediments, which LeConte calls 'the actual mountain-formation.' Hall's contribution to the problem, which, as LeConte well says, forms 'an era in geological science,' was to show the relation between mountain-chains and great accumulations of sediments; to illustrate this by the history of the palæozoic rocks of North America; and, moreover, to protest against the generally received theory that mountain elevations are due to local upthrusts; he, to use his own language, 'going back to the theories long since entertained by geologists relative to continental elevations.' That mountains were the remains of eroded continental areas had already been taught by Lesley, and long before by de Montlosier. It was left for Hall through a new way to lead us back to these views, but the whole theory of the cause of continental elevation was left by him where he found it. In my exposition of his views, I have only endeavored in addition to show in what manner a contracting globe and a solid nucleus may be related to the great facts of local subsidence and accumulation."

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"Professor LeConte declares that an important problem in geological dynamics remains in his opinion, unsolved, namely the cause of the 'great and widespread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata,' the last being that of the post-pliocene period. *Now*, it is precisely the *upward* movements of this kind which constitute the continental elevations of de Montlosier, Hall and myself, giving rise to plateaus, and by the partial erosion and denudation of these, to mountains. The *cause* of these continental elevations was not discussed by Hall, and is by LeConte declared to be unexplained; while such is the case 'the actual mountain formation,' to use his words, is still unaccounted for. That these gentle and widespread movements of oscillation are, however, in some way not yet clearly explained, connected with the contracting of the nucleus, and

the consequent conforming thereto of the envelope, we can scarcely doubt; or that the latter, from its nature and origin, must present great differences in constitution and in flexibility in its various parts. From this it might be expected that the movements imparted to the envelope alike by the process of secular cooling and contraction of the nucleus, and by the disturbance of the equilibrium of pressure consequent on the processes of erosion and sedimentation, would give rise to seemingly irregular oscillations, resulting in the depression or the elevation of considerable areas, constituting continental movements."

The two papers of the present writer will be found reproduced, the first in abstract, in his *Chemical and Geological Essays*, pp. 59–68, and 70–79.

In the latter he has adverted to the view adopted by Hall in his Address, that the crystalline rocks of New England, including the Green Mountains and the White Mountains, are altered palæozoic strata. This view which had been advocated by Mather, the brothers Rogers (though subsequently rejected by them), by Logan and others, was at that time accepted by the present writer; who has, however, long since rejected it as untenable, and maintained the prepalæozoic age of these mountains, and generally of the crystalline rocks of the Atlantic belt, a view which is now shared by most geologists. It is hardly necessary to say that the acceptance or rejection of this hypothesis of the palæozoic age of the crystalline rocks in question in no way affects Professor Hall's argument. The present view of the writer, now maintained by LeConte and most American Geologists, makes the rocks in question a part of the great prepalæozoic continental area lying to the north and east which Professor Hall supposed to be the source of the palæozoic sediments of the Appalachian basin.

SECTION A.

MATHEMATICS AND ASTRONOMY.

ADDRESS

BY

WILLIAM HARKNESS,

VICE PRESIDENT, SECTION A.

MEMBERS OF THE SECTION OF MATHEMATICS AND ASTRONOMY:—

TRANSITS of Venus usually occur in pairs; the two transits of a pair being separated by only eight years, but between the nearest transits of consecutive pairs more than a century elapses. We are now on the eve of the second transit of a pair, after which there will be no other till the twenty-first century of our era has dawned upon the earth, and the June flowers are blooming in 2004. When the last transit season occurred the intellectual world was awakening from the slumber of ages, and that wondrous scientific activity which has led to our present advanced knowledge was just beginning. What will be the state of science when the next transit season arrives God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present, and it seems a fitting occasion for noticing briefly the scientific history of past transits, and the plans for observing the one soon to happen.

When the Ptolemaic theory of the solar system was in vogue, astronomers correctly believed Venus and Mercury to be situated between the Earth and the Sun, but as these planets were supposed to shine by their own light, there was no reason to anticipate that they would be visible during a transit, if indeed a transit should occur. Yet, singularly enough, so far back as April, 807,

Mercury is recorded to have been seen as a dark spot upon the face of the Sun. We now know that it is much too small to be visible to the naked eye in that position, and the object observed could have been nothing else than a large sun spot. Upon the establishment of the Copernican theory it was immediately perceived that transits of the inferior planets across the face of the Sun must occur, and the recognition of the value of transits of Venus for determining the solar parallax was not long in following. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was first to work it out completely, and to him is usually assigned the honor of the invention. His paper, published in 1716, was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus of 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained.

When Kepler had finished his Rudolphine tables they furnished the means of predicting the places of the planets with some approach to accuracy; and in 1627 he announced that Mercury would cross the face of the Sun on November 7, 1631, and Venus on December 6 of the same year. The intense interest with which Gassendi prepared to observe these transits can be imagined when it is remembered that hitherto no such phenomena had ever greeted mortal eyes. He was destitute of what would now be regarded as the commonest instruments. The invention of telescopes was only twenty years old, and a reasonably good clock had never been constructed. His observatory was situated in Paris, and its appliances were of the most primitive kind. By admitting the solar rays into a darkened room through a small round hole, an image of the Sun nine or ten inches in diameter was obtained upon a white screen. For the measurement of position angles a carefully divided circle was traced upon this screen, and the whole was so arranged that the circle could be made to coincide accurately with the image of the Sun. To determine the times of ingress and egress, an assistant was stationed outside with a large quadrant, and he was instructed to observe the altitude of the sun whenever Gassendi stamped upon the floor. Modern astronomical predictions can be trusted within a minute or two, but so great did the uncertainty of Kepler's tables seem to Gassendi that he began to watch for the expected transit of Mercury two

whole days before the time set for its occurrence. On the 5th of November it rained, and on the 6th clouds covered the sky almost all day. The morning of the 7th broke, and yet there was no respite from the gloomy pall. Gassendi continued his weary watch with the sickening dread that the transit might already be over. A little before eight o'clock the sun began to struggle through the clouds, but mist prevented any satisfactory observation for nearly another hour. Towards nine the sun became distinctly visible, and turning to its image on the screen, the astronomer observed a small black spot upon it. It was not half as large as he expected, and he could not believe it was Mercury. He took it for a sun spot, and carefully estimated its position at nine o'clock, so that he might use it as a point of reference for the planet, if indeed he should be fortunate enough to witness the transit. A little later he was surprised to see the spot had moved. Although the motion was too rapid for an ordinary sun spot, the small size of the object seemed to forbid the idea that it was Mercury. Besides, the predicted time of the transit had not yet arrived. Gassendi was still uncertain respecting the true nature of the phenomenon when the sun again burst through the clouds and it was apparent that the spot was steadily moving from its original position. All doubt vanished, and recognizing that the transit, so patiently watched for, was actually in progress, he stamped upon the floor as a signal for his assistant to note the sun's altitude. That faithless man, whose name has been forgotten by history, had deserted his post, and Gassendi continued his observations alone. Fortunately the assistant returned soon enough to aid in determining the instant of egress, and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

After this success in observing Mercury, Gassendi hoped he might be equally fortunate in observing the transit of Venus on December 6, 1631. He knew that Kepler had assigned a time near sunset for first contact, but the tables were not sufficiently exact to forbid the possibility of the whole transit being visible at Paris. Alas, alas! these hopes were doomed to disappointment. A severe storm of wind and rain prevailed on December 4th and 5th, and although the sun was visible at intervals on the 6th and 7th, not a trace of the planet could be seen. We now know that

the transit happened in the night between the 6th and 7th, and was wholly invisible at Paris.

Transits of Venus can occur only in June and December, and as the two transits of a pair always happen in the same month, if we start from a June transit the intervals between consecutive transits will be 8 years, 105½ years, 8 years, 121½ years, 8 years, 105½ years, and so on. This is the order which exists now, and will continue for many centuries to come, but it is not always so. The path of Venus across the sun is not the same in the two transits of a pair. For a pair of June transits, the path at the second one is sensibly parallel to, and about twenty minutes north of, that at the first; while for a pair of December transits the parallelism still holds, but the path at the second one is about twenty-five minutes south of that at the first. Hence it happens that whenever Venus passes within about four minutes of the sun's centre at a June transit, or within about eight minutes at a December transit, she will pass just outside the sun's disk at the other transit of the pair, and it will fail. Thus the intervals between consecutive transits may be modified in various ways. If the first transit of a June pair fails, they will become 129½ years, 105½ years, 8 years, 129½ years, etc. If the second transit of a June pair fails, they will become 113½ years, 8 years, 121½ years, 113½ years, etc. If the first transit of a December pair fails, they will become 8 years, 113½ years, 121½ years, 8 years, etc. If the second transit of a December pair fails, they will become 8 years, 105½ years, 129½ years, 8 years, etc. And finally, if either the first or second transit of a pair fails both in June and December, they will become 113½ years, 129½ years, 113½ years, 129½ years, etc.

When Kepler predicted the transit of 1631, he found from his tables that at her inferior conjunction on December 4, 1639, Venus would pass just south of the sun, and therefore he believed the second transit of the pair would fail. On the other hand, the tables of the Belgian astronomer, Lansberg, indicated that the northern part of the sun's disk would be traversed by the planet. In the fall of 1639 this discrepancy was investigated by Jeremiah Horrocks, a young curate only twenty years old, living in the obscure village of Hoole, fifteen miles north of Liverpool, and he found, apparently from his own observations, that although

Kepler's tables were far more accurate than Lansberg's, the path of the planet would really be a little north of that assigned by Kepler, and a transit over the southern portion of the sun would occur. He communicated this discovery to his friend William Crabtree, and these two ardent astronomers were the only ones who had the good fortune to witness this, the first recorded transit of Venus.

Horrocks had great confidence in his corrected ephemeris of Venus, and it forbade him to expect the ingress of the planet upon the sun before three o'clock in the afternoon of Sunday, November 24, old style (December 4, new style); but as other astronomers assigned a date some hours earlier, he took the precaution to begin his observations on the 23rd. The 24th seems to have been partially cloudy, but he watched carefully from sunrise to nine o'clock; from a little before ten until noon; and at one o'clock in the afternoon; having been called away in the interval by business of the highest importance — presumably the celebration of divine service. About fifteen minutes past three he was again at liberty, and as the clouds had dispersed, he returned to his telescope and was rejoiced to find Venus upon the sun's disk, second contact having just happened. Only thirty-five minutes remained before sunset, but during these precious moments he made determinations of the position of Venus which are even yet of the highest value. Crabtree was less fortunate. At his station, near Manchester, there was but a momentary break in the clouds a quarter of an hour before sunset. This sufficed to give him a glimpse of the transit, and he afterwards made a sketch from memory.

The years sped swiftly by, and as the transit of 1761 approached, Halley's paper of 1716 was not forgotton, although he himself had long been gathered to his fathers. In deciding to what extent his plans could be followed, it was first of all necessary to know how nearly the real conditions would approximate to those he had anticipated. Passing over a paper by Trébuchet calling attention to errors in Halley's data, Delisle was first to point out the exact conditions of the transit, and the circumstances upon which the success of the observations would depend. In August, 1760, less than a year before the event, he published a chart showing that inaccurate tables of Venus had misled Halley, both as to the availability of his method, and in the selection of stations. The occasion could be more effectively utilized by a change of plan,

and Delisle considered it best to observe at suitably selected localities from many of which only the ingress, or only the egress, would be visible. Ferguson, in England, seems to have arrived independently at similar conclusions.

The two methods proposed respectively by Halley and Delisle have played so important a part in the history of physical astronomy that it will not be amiss to state briefly the distinction between them. The sun causes Venus to cast a shadow which has the form of a gigantic cone, its apex resting upon the planet, and its diameter continually increasing as it recedes into space. All the phenomena of transits are produced by the passage of this shadow cone over the earth, and as each point of the cone corresponds to a particular phase of a transit, any given phase will encounter the earth, and first become visible, at some point where the sun is just setting; and will leave the earth, and therefore be last visible, at some point where the sun is just rising. Between these two points it will traverse nearly half the earth's circumference and in so doing will consume about twenty minutes. The only phases dealt with by either Halley's or Delisle's method are the external and internal contacts, both at ingress and at egress. Delisle's method consists in observing the times of contact at stations grouped about the regions where either ingress or egress is soonest and latest visible. The longitudes of the stations must be well determined, and then by combining them with the observed times of contact the rate at which the shadow cone sweeps over the earth becomes known, and from it the solar parallax results. At many of the stations best suited for Delisle's method, only the beginning or only the ending of the transit will be visible; but for the application of Halley's method, both the beginning and the ending must be seen. The theory of the latter method is so complicated that it is difficult to explain it briefly and at the same time accurately; but the following considerations will suffice to indicate its nature. The duration of a transit at any point on the earth's surface depends partly upon the length of path, and partly upon the velocity, of that point while within the shadow cone. The length of path is affected by the latitude of the point, and the velocity by the earth's diurnal motion, which in some regions accelerates, and in others retards, the progress of the shadow. The result is that throughout one-half the earth's surface the duration of the transit is lengthened, while throughout the other

half it is shortened; the maximum lengthening and shortening occurring at the respective poles of the hemispheres in question. Although these poles are not situated at the extremities of the earth's axis, it usually happens that one of them is shrouded in night; but upon the sunlit side of the earth, from which alone observations can be made, localities may exist at some of which the duration of the transit will be twenty minutes or more greater than at others. This inequality is the condition upon which Halley's method depends, and when such localities are accessible it may be advantageously applied. Briefly then, Halley's method consists in observing the duration of a transit at two or more stations so selected as to give durations of widely different lengths; while Delisle's method consists in employing a common standard time to note the instant when the transit begins, or ends, at two or more stations so chosen as to give very different values for that instant.

The transit of 1761 was visible throughout Europe and was well observed by astronomers in all parts of that continent. Besides this, England sent expeditions to St. Helena and to the Cape of Good Hope; and English astronomers observed at Madras and Calcutta; French astronomers were sent to Tobolsk, Rodriguez, and Pondicherry; Russians to the confines of Tartary and China; and Swedes to Lapland. No less than 117 stations were occupied by 176 observers; and of these, 137 published their observations. When this mass of data was submitted to computation, the result was far from satisfactory. Values of the solar parallax were obtained ranging from 8.49 seconds to 10.10 seconds; and in their disappointment the astronomers of the eighteenth century concluded that too much reliance had been placed upon Delisle's method.

The transit of 1769 drew on apace; and, to avoid a repetition of the fancied mistake of 1761, attention was directed almost exclusively to Halley's method. The conditions of the transit were carefully discussed by Hornsby in England, and by Lalande and Pingré in France; and it was found that its duration would be greatest in Lapland and Kamschatka, and least in the Pacific Ocean, California and Mexico. Astronomers were dispatched to all these regions. England sent the famous Capt. Cook to Otaheite; France sent Chappe to California; the King of Denmark sent Father Hell to Lapland; and in addition numerous observations were made in Europe, North America, China, and the East Indies.

The preparations were most elaborate, and the result better than in 1761, but still not satisfactory. The black drop and other distortions disturbed the contacts in this transit as they had done in the previous one, and the values of the parallax deduced by the best computers ranged from 8.43 seconds to 8.85 seconds.

Thus the matter rested till 1825 and 1827 when Encke published abstracts of his discussion of the transits of 1761 and 1769, from which he deduced a parallax of 8.58 seconds. This discussion was not printed in full till 1835, when it immediately commanded the attention of astronomers, and its result, which Encke had modified to 8.57 seconds, was universally accepted for more than a quarter of a century. As time wore on, certain gravitational investigations led to a strong suspicion that the sun's distance had been overestimated by at least three million miles, and the observations of Mars at its opposition in 1862 converted this suspicion into a conviction. The eighteenth century transits were again rediscussed and a parallax of 8.83 seconds was found from them by Powalky in 1865, and 8.91 seconds by Mr. E. J. Stone in 1868. Newcomb's paper, in 1867, also produced a marked impression.

The transit of 1874 was then approaching, and in the discussion as to how it should be utilized Halley's and Delisle's methods once more played a prominent part. It was recognized that the uncertainty in the observed times of contact of the eighteenth century transits was largely due to the black drop, and the causes of that phenomenon were carefully considered. Among them, most astronomers believed that irradiation played an important, if not the principal, part; but at the same time there was a general feeling that the telescopes of a century ago were bad, and that the magnificent instruments of the present day would give better results. In view of all the circumstances it was determined that the contacts should be observed with equatorially-mounted achromatic telescopes of from 4 to 6 inches' aperture, or with reflectors of not less than 7 inches' aperture, and that magnifying powers of from 150 to 200 diameters should be employed. The Germans and Russians adopted heliometers of about three inches' aperture for making exact determinations of the positions of Venus during transit, but other nations did not follow their example.

Photography, an agency undreamed of in the eighteenth century, was also available, and all saw the desirability of employing it; but there was much difference of opinion as to how this should be

done. The European astronomers preferred instruments modelled upon the Kew photoheliograph, whose objective has 8.4 inches aperture and 50 inches focus, giving an image of the sun 0.482 of an inch in diameter, which is enlarged by a secondary magnifier to 3.93 inches. On the other hand, the American astronomers contended that photographs taken with such instruments would be affected by troublesome errors due to the secondary magnifier, that position angles could not be measured from them accurately enough to be of any use, and that it would be exceedingly difficult to determine the exact linear value of a second of arc. They advocated the use of horizontal photoheliographs, which are free from all these disadvantages; and the instruments which they adopted had apertures of 5 inches, and focal distances of $38\frac{1}{2}$ feet, giving images of the sun slightly more than 4 inches in diameter. Notwithstanding this radical difference of opinion respecting the best form of photoheliograph, the astronomers of the old and new worlds were in perfect accord as to how the instruments should be employed. Between the first and second contacts, and again between the third and fourth contacts, photographs about five minutes square, showing the indentation cut by the planet into the sun's limb, were to be taken at intervals of a few seconds; and from these it was hoped the true times of contact could be deduced with great accuracy. Between the second and third contacts, pictures of the entire sun were to be taken at short intervals, and the positions of Venus relatively to the sun's centre were to be obtained from them by subsequent measurements. In the latter case, the photoheliograph took the place of a heliometer, and was superior to that instrument in its power of rapidly accumulating data.

The question of instrumental outfit having been disposed of, stations were selected, and parties dispatched to almost every available point. The United States, England, France, Germany, Russia, Holland,— in short, nearly all the nations of the civilized world,— took part in the operations. The weather was not altogether propitious on the day of the transit, but nevertheless a mass of data was accumulated which will require years for its thorough discussion. When the parties returned home the contact observations were first attacked, but it was soon found that they were little better than those of the eighteenth century. The black drop, and the atmospheres of Venus and the Earth, had again produced a series of complicated phenomena, extending over many

seconds of time, from among which it was extremely difficult to pick out the true contact. It was uncertain whether or not different observers had really recorded the same phase, and in every case that question had to be decided before the observations could be used. Thus it came about that within certain rather wide limits the resulting parallax was unavoidably dependent upon the judgment of the computer, and to that extent was mere guesswork. Attention was next directed to the photographs, and soon it began to be whispered about that those taken by European astronomers were a failure. Even yet I am not aware that the Germans have published anything official on the subject; but the English official report has appeared, and it frankly declares that "after laborious measures and calculations it was thought best to abstain from publishing the results of the photographic measures as comparable with those deduced from telescopic view." From the way in which these photographs were taken, Sir George Airy saw that they could not yield position angles of any value, and therefore differences of right ascension and declination could not be determined from them; but they did seem capable of giving the distance between the centres of Venus and the sun with considerable accuracy. Upon trial this proved not to be the case. No two persons could measure them alike, because "however well the sun's limb on the photograph appeared to the naked eye to be defined, yet on applying to it a microscope it became indistinct and untraceable, and when the sharp wire of the micrometer was placed on it, it entirely disappeared." In short, the British photographs are useless for the present, but Sir George Airy hopes that in the future some astronomer may be found who will be capable of dealing with them.

We turn now to the American photographs. They present a well defined image of the sun about 4.4 inches in diameter, and are intended to give both the position angle and distance of Venus from the sun's centre. A special engine was at hand for measuring them, but when they were placed under the microscope only an indistinct blur could be seen. Here again was the same difficulty which had baffled the English, but fortunately its cause was soon discovered. The magnifying power of the microscope was only 37½ diameters, which seemed moderate enough, but was it really so? The photographic image of the sun was about 4.4 inches in diameter, and this was magnified 3.31 times by the objective of the microscope, thus giving an image 14.56 inches in diameter.

To yield an image of the same size, a telescopic objective would require a focus of about 1563 inches, and if the eye-piece of the microscope, which had an equivalent focus of 0.886 of an inch, were applied to it, a power of 1764 diameters would be produced. This then was the utterly preposterous power under which the image of the sun was seen when the photograph was viewed through the microscope,and no useful result could be expected from it. Means were immediately provided for reducing the power of the microscope to 5.41 diameters, and then the photograph seen through it appeared as the sun does when viewed through a telescope magnifying 255 diameters. After this change all difficulty vanished, and the photographs yielded excellent results. The measurements made upon them seem free from both constant and systematic errors, and the probable accidental error of a position of Venus depending upon two sets of readings made upon a single photograph is only 0.553 of a second of arc. To prevent misunderstanding it should be remarked that this statement applies only to pictures taken between second and third contact, and showing the entire sun. The small photographs taken between first and second contact, and again between third and fourth contact, proved of no value.

These investigations consumed much time, and before the result from the American photographs was generally known, an international convention of astronomers was held in Paris to consider how the transit of 1882 should be observed. The United States was not represented at this conference, and guided only by their own experience, the European astronomers declared that photography was a failure and should not be tried again. They knew that the contact methods are attended by difficulties which have hitherto proved insurmountable, but under the merciless pressure of necessity, they decided to try them once more. Unfettered by the action of the Paris Conference, the United States Transit of Venus Commission took a very different view of the case. Its members knew that the probable error of a contact observation is 0.15 of a second of arc, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. They also knew that the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only 0.55 of a second of arc. If

the sun is visible for so much as six minutes between the second and third contacts, by using dry plates thirty-six negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. These were the reasons which led the American Commission to regard photography as the most hopeful means of observation, and thus it happens that the astronomers of the old and new worlds differ radically respecting the best means of utilizing one of the most important astronomical events of the century. The Europeans condemn photography, and trust only to contacts and heliometers; the Americans observe contacts because it costs nothing to do so, but look to photography for the most valuable results.

In 1716, Halley thought that by the application of his method to the transit of 1761, the solar parallax could certainly be determined within the five hundredth part of its whole amount. Since then, three transits have come and gone, and the contact methods have failed to give half that accuracy. From the photographic method, as developed by the U. S. Transit of Venus Commission, we hope better things, and perhaps fifty years hence its results may be regarded as the most valuable of the present transit season. In 1874, as in 1761, exaggerated views prevailed respecting the value of transits of Venus, but no competent authority now supposes that the solar parallax can be settled by them alone. The masses of the Earth and Moon, the moon's parallactic inequality, the lunar equation of the earth, the constants of nutation and aberration, the velocity of light, and the light equation, must all be taken into account in determining the solar parallax, and it cannot be regarded as exactly known until the results obtained from trigonometrical, gravitational, and phototachymetrical, methods are in perfect harmony. It may be many years before this is attained, but meanwhile practical astronomy is not suffering. Its use of the solar parallax is mainly confined to the reduction of observations made at the surface of the earth to what they would have been if made at the Earth's centre; and for that, our present knowledge suffices. The real argument for expending so much money upon transits of Venus is that being an important factor in determining the solar parallax, their extreme rarity renders it unpardonable to neglect any opportunity of observing them. Let us do our whole duty in this matter that posterity may benefit by it, even as we have benefited by the labors of our predecessors.

PAPERS READ.

New views of Mr. George H. Darwin's Theory of the Evolution of the Earth-Moon System, considered as to its bearing on the question of the duration of Geological Time. By Samuel Haughton, of Dublin, Ireland.

It has been tacitly assumed, even so far back as the times of Newton and Clairaut, that the earth and planets have passed through a liquid condition (owing to former great heat) before assuming the solid condition, which some of them, at least, now possess.

Laplace, in his nebular hypothesis, also assumes the former existence of this liquid condition, and it is openly asserted by all geologists who believe that the earth consists of a solid crust (more or less thick), reposing upon a fluid or viscous nucleus.

It has been proved by Sir William Thomson, following out the views of the late Mr. Hopkins, that the present condition of the earth, taken as a whole, is such that it must be regarded as being more rigid than glass or steel, possibly more rigid than any terrestrial substance under the surface conditions of pressure.

The following considerations show that it may be fairly doubted whether the earth or any other planet ever existed in a fluid condition.

1. The possibility of the equilibrium of the rings of Saturn, on the supposition that they are either solid or liquid, has been more than doubted, and the most probable hypothesis respecting them is, that they consist of swarms of discrete meteoric stones.

2. It is difficult to understand the low specific gravity of Jupiter and the other outer planets, on the supposition that they are either solid or liquid, for we know of no substance light enough to form them.[1] If the outer planets consist of discrete meteoric

[1] The force of this argument could not be felt before the revelations of the spectroscope, because at that time there was no proof that the whole universe was composed of the same simple substances, and those very limited in number.

stones moving around a solid or liquid nucleus, the difficulty respecting their specific gravity would disappear.

3. The recent researches connecting the November, the August, and other periodic swarms of shooting stars with comets tend in the direction of showing that comets, in cooling, break up into discrete solid particles (each no doubt having passed through the liquid condition); and that probably the solar nebula cooled in like manner into separate fiery tears, which soon solidified by radiation into the cold of space.

4. Mr. Huggins's recent comparisons of the spectroscopic appearances of comets and incandescent portions of meteoric stones, showing the presence in both of hydrocarbon and nitrogen compounds, confirm the conclusions drawn from the identity of the paths of comets and meteoric periodic shooting stars.

5. Mr. H. A. Newton, in a remarkable paper read before the Sheffield Meeting of the British Association (1879), showed the possibility (if not probability) of the asteroids being extinct comets, captured and brought into the solar system by the attraction of some one or other of the outer large planets, and permanently confined in the space between Mars and Jupiter, which is the only prison cell in the solar system large enough to hold permanently such disorderly wanderers.

In the same paper, Professor Newton threw out the idea that some of the satellites of the large planets might also be of cometary origin.

From all these and other considerations, it is therefore allowable to suppose that the earth and moon, when they separated from the solar nebula, did so as a swarm of solid meteoric stones, each of them having the temperature of interstellar space; *i. e.*, something not much warmer than 460° F. below the freezing point of water.

Mr. George H. Darwin has shown, admirably, how the earth-moon system may have been developed from the time when the earth-moon formed one planet revolving on its axis in a few hours, to the present time, when the earth and moon (in consequence of tidal friction) have pushed each other asunder to a distance of sixty times the radius of the earth.[2]

In his paper on the tidal friction of a planet[3] (supposed viscous and under the influence of bodily tides caused in it by an external

[2] Proceedings of Royal Society, 19th June, 1879.
[3] Phil. Trans., 1881, Part ii, p. 494.

body such as the moon), Mr. Darwin has found a remarkable equation of condition, which may be thus expressed:

$$d(\sqrt{r}) \propto \frac{\Psi dt}{r^6} \tag{1}$$

where r = distance between centres of earth and moon.
t = time elapsed from a fixed epoch.

$$\Psi = \frac{p(n-\Omega)}{1+p^2(n-\Omega)^2} \tag{2}$$

n = angular velocity of earth's rotation.
Ω = angular velocity of moon's orbital revolution.
p = quantity varying inversely as the viscosity of the planet.

The extreme interest of equation (1) consists in the appearance of the inverse sixth power of the distance.

As the function Ψ varies very slowly, we find by integration, for any portion of time during which Ψ may be regarded as constant

$$t = Ar^{\frac{13}{2}} + B, \tag{3}$$

a most unexpected and remarkable result.

Upon reading Mr. Darwin's papers, my mind turned to a problem with which I was familiar, viz.: the retardation of the earth's rotation produced by the lunisolar tide exerted upon the ocean supposed collected in an equatorial canal, the moon and sun having no declination, and I readily found an equation to express the evolution of the earth-moon system, on the foregoing hypothesis as to friction.

This equation is the following:

$$d(\sqrt{r}) \propto \frac{f\Phi dt}{r^6} \tag{4}$$

where

$$\Phi = \frac{V_0(n-\Omega)}{\{4V_0^2(n-\Omega)^2 - k^2\}\sqrt{4(n-\Omega)^2+f^2}} \tag{5}$$

f = coëfficient of friction supposed proportional to relative velocity.
k varies inversely as r^3.
V_0 = velocity at earth's equator.

This leads, as in Mr. Darwin's hypothesis of viscous earth, to the integral

$$t = A'r^{\frac{13}{2}} + B' \tag{6}$$

The form of the functions Ψ and Φ is similar, as both ascend by odd powers of $(n-\Omega)$ and vanish when $n=\Omega$, that is to say, at the beginning and end of the evolution by friction of the earth-moon system.

It is quite clear, therefore, that the remarkable expression (1) found by Mr. Darwin, is not peculiar to his special hypothesis of a viscous earth, but can be deduced equally well from the totally distinct hypothesis of an absolutely rigid earth retarded by the tidal action of a liquid ocean.

I was led by this result to consider the case of the earth-moon separating (as I believe they did) from the central solar mass, in the form of a swarm of discrete masses of meteoric iron and stone, each one having the temperature of the cold of interstellar space, or not much above it. Translating this conception into mathematical language, I find that the equation of continuity belonging to the hydrodynamical theory applies equally well to the meteoric theory, viz.:

$$vy=v'y' \qquad (7)$$

where v, v' are the velocities at any two points, and y, y' are the depths of the ocean or meteoric swarm at the same points.

The depth of the swarm or ocean, without jostling or friction will be least under the moon, and greatest at right angles to the moon, and the velocities will be inversely. Hence the chances of jostling among the meteorites, when disturbed by the moon's tidal action will be proportional to the velocity, being greatest where the velocity is greatest and the area of passage least, and *vice versa*.

This consideration reduces the meteoric problem to that of the hydrodynamical problem, with a friction proportional to the velocity, and gives equations, in all respects similar to those derived by Mr. Darwin, from the hypothesis of a viscous earth.

On the meteoric hypothesis, if the jostling of the stones be slow they may cool almost as fast as they are heated and the result will be a cool earth and almost indefinite time at the disposal of geologists.

THE PARALLAX OF α LYRÆ AND 61 CYGNI. By ASAPH HALL, of Washington, D. C.

THE question of the parallaxes of the fixed stars, or their distances from the earth, was for a long time a subject of speculation among the ancient astronomers. Very little, however, could be done towards answering this question until the true theory of our solar system had been discovered, and the invention of the telescope had made possible more accurate astronomical observations. Even when these things had been done there remained other questions to be solved, such as those of aberration and refraction, before astronomers could reduce their observations correctly and bring the problem of stellar parallax to the test of exact measurement. All these reductions are easy to us now, but the astronomer who lived two hundred years ago found himself in a maze of perplexities, and nearly a century passed away before these difficulties were overcome so far as to make feasible the determination of the distances of the stars.

When it was seen that the motion of the planets around the sun is undoubtedly the true theory of our solar system, and that we have a long base line to measure from, astronomers were not slow to undertake observations for stellar parallax. One of the earliest observers was Rothmann, astronomer for William of Hesse, in Germany. Rothmann found a difference of 2′ in the latitude of his observatory from observations made in summer and in winter, and hence he inferred that the stars have an annual parallax, which confirmed the Copernican theory that the earth is in motion around the sun. But Tycho Brahe, the best observer of those times, declared that his own observations, made with the best instruments and with the greatest care, showed no such change of latitude; and hence Tycho drew the conclusion that the stars have no parallax, and that the theory of the motion of the earth around the sun is false. One of the most ingenious early attempts to investigate stellar parallax was that of Dr. Robert Hooke of London in 1669; but Hooke seems to have been a man of unsteady purpose, and failed to carry out his plan of observation, so that his work came to nothing. The next remarkable attempt was by the English astronomer, Flamsteed, in 1689–1697. Flamsteed made very careful determinations of the zenith distances of Polaris at its upper and lower culminations at different

times of the year, and correcting these for precession he found that the results showed a periodical change, which he ascribed to parallax. But as soon as Flamsteed published his observations, it was pointed out by Cassini and others that the periodical changes could not be explained by a parallax of the star. It is curious, however, that no serious attempt was made to account for Flamsteed's results, the most careful and accurate that had been obtained up to his time. These results contain, in fact, the data for a very good determination of the constant of aberration, as was shown by C. A. F. Peters in 1846, and the value of this constant found from Flamsteed's observations is in error only a quarter of a second of arc.

Another generation of astronomers continued their attempts at the determination of stellar parallax, from observations of the zenith distances of the stars, but with no results. Many of these observations show periodical changes, which gave rise to various theories; but probably all these changes may be explained by variations in the instruments, or by incomplete reduction of the observations. In fact, no trustworthy determinations of parallax were possible until after the discovery of the aberration of light; and Bradley's discovery of this phenomenon, in 1728, put an end to most of the crude attempts. The coëfficient of aberration is 20.″4; and although the correction for aberration is zero at the time of the maximum of parallax, yet the motion of the earth in a single day will introduce a correction for aberration amounting to one-third of a second of arc, a quantity comparable with the parallaxes of the stars. Bradley was one of the few astronomers of the past century who knew the value of accurate and definite observations, and from his own work he drew the conclusion that the parallaxes of the stars must be less than a second of arc. Bradley's labors seem to have shown astronomers the great practical difficulty of this problem of stellar parallax, and few further attempts were made until toward the beginning of the present century when the instruments had been much improved.

The Italian astronomer, Piazzi, in 1792–1804, found parallaxes of several of the bright stars, amounting in some cases to 3″ or 4″, but his results are certainly wrong. About this time Brinkley, the astronomer Royal of Ireland, devoted fourteen years of careful and intelligent observation to the question of stellar parallax. This is one of the most remarkable researches of the kind ever

made. The values of parallax found by Brinkley amount in some cases to 2″; and his work led to a long dispute between himself and Pond, the astronomer Royal at Greenwich. Although Brinkley was a skilful astronomer, and a man whom no amount of labor could deter, yet we must say that his results for parallax are mostly erroneous. However, the great difficulty of dealing with this question, by means of absolute measures made with divided circles, seems to have been brought out clearly by Brinkley's observations and their subsequent criticism and discussion.

The most elaborate and successful attempt that I know of to determine the parallaxes of the stars by such absolute measurements is that made by C. A. F. Peters at Pulkowa in 1842–'43. In the care and skill of observation, and completeness of reduction, this work has never been surpassed. Peters finds positive values of the parallaxes of seven of the eight stars observed, and in the case of the single negative parallax the value is less than a tenth of a second. The parallax of 1830 Groombridge found by Peters is $0''.226 \pm 0''.141$; a value much less than that found by a differential method, and subsequent investigations have proved the correctness of Peters' result.

The absolute determination of so small a quantity as a stellar parallax is an extremely difficult matter, since the observations must be extended over a year, during which the instrument is exposed to great changes of temperature which act on the circle, and which may influence the reductions. The divided circle may be avoided by using a transit instrument in the prime vertical, although the field of observation is in this way restricted to fewer stars. If Struve's method of reversing the instrument in each vertical be used, and stars are chosen that pass near the zenith, two of the instrumental corrections are almost wholly eliminated; but the third, or level correction, comes in with nearly its full value. With the excellent spirit levels made by Repsold, it was found by Struve that the probable error from this source was only a few hundredths of a second of arc. For stars of the first and second magnitudes that can be observed at all seasons of the year, this instrument promises good results, and in the absolute determinations of parallax, and in the investigations of the constants of aberration and nutation, it may yet fulfil the expectations of Struve.

It was long ago pointed out by Galileo, that if two stars are near

each other, and one of them is bright and the other faint, probably these stars are at greatly different distances from us; and that differential observations of such stars would lead to a knowledge of their difference of parallax. Huygens applied this method to some of the binary systems, and of course found no parallax. A century afterwards Herschel also applied this method to some of the binary stars, and again no parallax was found; but by continuing his observations Herschel found that many of these stars were in motion around each other, and recognizing that such stars must be at nearly the same distance from us, he saw that no parallax could be found from such observations. In some respects, however, this method of Galileo presents great advantages. The observations being merely differential they can be made with micrometers of various kinds, and with greater accuracy and ease than absolute determinations. The reductions are also far simpler and more easily computed, and we avoid in a good degree the effects produced by changes of temperature. It is true that by this method we get only the difference of the parallaxes of the stars, and there will always remain some doubt concerning the value of the real parallax. But if a number of different stars be used for comparison, and the same value of the parallax be found from the various stars, we may fairly conclude that the parallax belongs to the principal star. A practical difficulty is met with here in the fact that most of the stars that have large proper motions, and those that for other reasons are such as we would observe for parallax, have no suitable comparison stars within reach of the micrometers commonly in use; that is, if we follow the method of measuring angles of position and distance, which is perhaps the best. In some cases this difficulty may be avoided by observing only the difference of declination, when the coëfficient of parallax in declination is not too small, although by doing this we give up the advantage of an independent determination of the parallax from the angle of position.

In 1862 a series of observations of α Lyræ was begun at the U. S. Naval Observatory by Professor J. S. Hubbard, and this series was continued by Professor S. Newcomb and his assistants until April, 1867. Each complete observation consists of four transits over seven wires, the instrument being reversed in each vertical. This star passing only a quarter of a degree south of our zenith, the correction for azimuth is nearly zero, and the colli-

mation being eliminated by the method of observing, there remained only the correction for the level, which was determined by an excellent instrument made by Würdemann of Washington. This long and careful series of observations proved, however, of no value for determining the parallax of this star. Some unknown disturbance acted in such a way that the parallax comes out negative. This unfortunate result led me to make an attempt to find the parallax of this star by the differential method, while our observatory remains on its present site. These observations were begun May 24, 1880, and were finished July 2, 1881. As the famous star 61 Cygni could be observed without spending much more time, observations on this star were begun Oct. 24, 1880, and ended Dec. 7, 1881. These observations were the simplest possible, being measures of the difference of declination from a comparison star of the tenth magnitude. My observations of α Lyræ consist of two series, one with a bright field and dark wires, and the other with a dark field and bright wires; those of 61 Cygni were made with the dark wires only. Before beginning these measurements an arrangement was attached to the tube of the micrometer for adjusting the stellar focus, so that this adjustment could be deliberately and carefully made, and during the work care was taken that this adjustment should be always exact. In the case of our 26-inch refractor the tube of the micrometer is shifted one-thirtieth of an inch during the year, it being pushed in during the cold weather and drawn out in summer. It has been assumed in all our previous reductions that the coëfficient of temperature for the screw of our micrometer is zero. To test this assumption, which would have an important influence on the reductions, at the same time that the observations for parallax were going on a series of measurements was made on a pair of stars in the cluster h Persei. These measurements were not reduced until the observations for parallax were finished, and on account of the previous result of a zero coëfficient for temperature, the measurements were not so numerous as they might otherwise have been; still they show a decided correction for change of temperature.

The observations for parallax were arranged so that the micrometer and the wires were reversed on each night, and at the end of an observation the wires were restored to their first position. Each of these observations being reduced to a chosen epoch by

correcting for proper motion, precession, nutation and aberration, gives an equation of condition of the form

$$x + by + cz + du + n = o,$$

where x is the correction to the assumed difference of declination, y the correction to the assumed value of the annual proper motion, z the difference of the constants of aberration for the two stars, u the difference of the parallaxes of the stars, and n is the residual between the observed difference of declination and an assumed value. The assignment of weights to such equations is a delicate matter, and whenever possible I have preferred to give the weight unity to each of them, notwithstanding the fact that the observations were made under very different conditions of the images. The solution of each set of equations by the method of least squares gives the following values of the parallax:

α Lyræ.

Dark wires, $\pi = 0''.1556 \pm 0''.00764.$
Bright " $\pi = 0.2080 \pm 0\ .00827.$

Taking the mean of these results by weight, we have the final result:

$$\pi = 0''.1797 \pm 0''.00561.$$
Time of light = 18.11 Julian years.

61 Cygni.

Dark wires, $\pi = 0''.4783 \pm 0''.01381.$
Time of light = 6.803 Julian years.

The substitution of these values in the equations of condition shows that the greatest residual in the case of α Lyræ is for the dark wires 0″.120; and for the bright wires it is 0″.160, there being 69 equations in the first case and 59 in the second. The observations of 61 Cygni are not quite so good, the greatest residual here among the 66 equations being 0″.209.

The star 61 Cygni has one of the largest and best determined parallaxes that we know of, since a recent investigation by Dr. Elkin has reduced that of α Centauri to nearly the same value. This quantity being half a second of arc, it can be seen easily with a good telescope. But a tenth of a second is so small that one

can hardly distinguish it even with the highest magnifying powers, except with remarkably steady and distinct images. To determine a parallax of this amount we must trust therefore that in a large series of measurements the influence of the parallax will be shown in the mean result. And my own experience has convinced me that such will be the case, if the observer be careful, and has practised so long that his habit of observing will not change during the period of observation.

On a method of reducing different Catalogues of Stars to a homogeneous system. By William A. Rogers, of Cambridge, Mass.

[ABSTRACT.]

When two catalogues of stars which have been independently observed are compared, it will be found that there are certain systematic deviations *inter se* which are functions either of the right ascension or of the declination, or of both the right ascension and the declination. If all the stars in the two catalogues are compared for each hour of right ascension and the mean of the deviations for each hour be taken, it will be found that this mean is never a constant. So also the mean of the residuals obtained by combining the stars in groups, arranged in the order of declination, will be found to vary with the declination.

Now our knowledge of the motion of the solar system in space depends upon our knowledge of the proper motions of the stars composing the stellar system. Since all the catalogues of stars which have ever been made, have variations from any normal system assumed, it is necessary to reduce each catalogue to a common system before the observations can be employed in the determination of the proper motion.

It is the common practice to arrange the stars in the order of right ascension and to take the mean of the residuals from the normal system chosen for each hour. Since these residuals involve the accidental errors of observation, it is then necessary to obtain a system of mean corrections which shall be as nearly

free as possible from accidental errors. There are two methods of doing this—

I. The Analytical Method.

If we have a series of residuals $\Delta\alpha$ arranged in the order of right ascension, which are strictly circular functions, we may always assume:

$$\Delta\alpha = \text{a constant} + m \sin\alpha + n \cos\alpha + m' \sin 2\alpha + n' \cos 2\alpha \text{ etc.}$$

From the solution of a series of equations of this form the values of m n m' and n' can be found by the process of least squares. Hence the value of $\Delta\alpha$ becomes known for any right ascension.

II. The Graphical Method.

If we assume any given unit in right ascension as a horizontal argument, and an aliquot part of $\Delta\alpha$ as a vertical argument, it is obvious that points representing $\Delta\alpha$ may be laid off which will bear a definite relation to a fixed horizontal line. If a smooth curve be drawn through these points values of $\Delta\alpha$ nearly representing observation may be derived for any right ascension by reading off the vertical coördinate passing through the curve at this point.

Now when the systematic deviations of any catalogue from the normal system have been found by either of these methods and have been applied to the positions of the catalogue, it will still nearly always be found that there are remaining residuals which are functions of the declination. These residuals can be treated either analytically or graphically by the methods just described.

But when the chosen catalogue has been reduced to the normal system by the application of both of these classes of systematic corrections, *i. e.*, those depending on the right ascension, and those depending on the declination, obtained independently, there are several catalogues which still show a system of residuals which are functions of both the right ascension and of the declination.

It is easy to see why this must be so, to a certain extent. Nearly all modern catalogues depend upon Bessel's constants of precession. A few, however, depend upon Struve's constants.

In order to reduce the annual variation for Bessel's constants to those of Struve, we have: —

$$\text{Struve} = \text{Bessel} + .001123^{s} + 000129^{s} - \text{tang}\,\delta \sin\alpha$$

It will be seen that the variable part of this reduction depends upon both a and δ.

Both of the methods described for comparing different catalogues with a normal catalogue are open to the objection that the coördinates of any given star when referred in this way to a normal system, may differ by a small amount from the values of the coördinates derived by a direct reduction of the instrumental constants from the fundamental stars of the system. I estimate the uncertainty arising in this way to be about $\pm 0.015^s$ in Δa and $\pm 0.20''$ in $\Delta\delta$.

In the reduction of the zone stars observed at the Harvard College Observatory, the instrumental constants from 1871 to 1876 have been recomputed from the final positions of the fundamental catalogue upon which it is to be based, viz., upon the catalogue of fundamental stars in publication, the primary reductions having been made with the provisional places of these stars. When, therefore, the final catalogue is completed, the data will be at hand for a definite determination of the amount of this uncertainty. But since this reduction is entirely impracticable in the case of the catalogues already published finally, some method of reducing different catalogues to a homogenous system is an absolute necessity. Admitting the necessity — *that method is to be preferred which will reduce the residuals Δa and $\Delta\delta$ to a minimum, whatever the order or the limits of the groups into which they may be divided.*

The method proposed is as follows: —

(a) The residuals are first arranged in the order of declination. The mean values for any group represent nearly the corrections for the mean declination of that group, and for twelve hours of right ascension.

(b) The residuals for each group minus the mean value for that group, are then arranged in the order of right ascension, and a graphic curve is drawn through the points representing these values. Each curve will give a series of values for the even hours of right ascension, which are arranged in vertical columns, the horizontal argument being the mean declination. It will be seen that the values in the vertical columns are derived from the same curve, while those in the horizontal columns are derived from different curves. In order to connect the different groups in declination a curve is drawn through the points represented by the residuals in the horizontal columns. This will, of course, disturb to a slight

extent, the values already found in the vertical columns, but they can in turn be rectified in the way already described. A second approximation will ordinarily give smooth curves for both the vertical and the horizontal arguments.

(*c*) On account of the limited number of residuals which usually compose the groups arranged in the order of declination, there is danger of introducing systematic errors in drawing the graphic curves. Hence, after the sum of the two residuals already found has been subtracted from the original residuals, the new values are arranged in the order of right ascension and a smooth curve is drawn through the points thus found, giving the means of obtaining the slight corrections which still remain.

This method has been followed in a paper printed in Vol. X of the Memoirs of the American Academy of Arts and Sciences on "A comparison of the Harvard College Observatory Catalogue of Stars for 1875.0 with the fundamental systems of Anwers, Safford, Boss and Newcomb."

On the Performance of a New Form of Level invented by Mr. John Clark of the U. S. Coast Survey. By William A. Rogers, of Cambridge, Mass.

[ABSTRACT.]

Experienced observers have often expressed the opinion that the level as ordinarily constructed cannot be regarded as an instrument of precision. It will be admitted on every hand that it is not made by a scientific method. In fact, the method of construction is of the most crude kind. The maker chooses a glass cylinder as uniform in thickness and density as he can obtain and proceeds to grind an interior curve of unknown radius with an emery and a polishing tool. He has no means of determining the exact radius by observation. He is unable to test the equality of the curvature on each side of the centre, until after the cylinder has been filled with a fluid and after the ends have been hermetically sealed. Even then the test is only a relative one. It is of the

same kind as the observations which are made with the completed level.

As the result of many years of experience I have reached the conclusion that an astronomical level can be regarded as an instrument of precision, only when it is subjected to adequate tests during the series of observations in which it is employed. A given level may measure small angles with great precision, but the same level under different conditions may give untrustworthy results.

Even with an instrument which has no graduated circle, an adequate level trier can be constructed by placing a graduated scale at a known focal distance. In the case of the Meridian Circle of Harvard College Observatory the level is placed in Y's attached to the cube of the instrument placed parallel with the optical axis.

In order to show the necessity for repeated and continuous observations for the determination of the value of one division of the level, I record here my experience with the companion level of the instrument known as the Russian Transit. It was made in the workshop of the Pulkowa Observatory.

I began the observations about eight o'clock in the morning of Nov. 13, by comparing the readings of the circle with bubble first at the middle of the tube and then at the extreme end. Proceeding in this way with each five divisions in succession I was surprised to find, not only a continued diminution of the value of one division, but a well defined shifting of the zero of the level. By noon I had nearly completed the examination for the first half of the divisions. I then opened the shutters for an observation of the sun. After an interval of ten minutes, observations with the level were resumed when it was found that the value of one division, determined from the same space as before, had increased by one-fourth of its mean value. It will be sufficient to give in illustration the results of the observations on three days.

1881	Nov. 13.		Nov. 14.		Nov. 15.	
Shutters closed.	1 div = 1.76″	Th. = 38°	1 div = 1.20″	Th. = 40°	1 div = 2.06″	Th = 47°
Shutters open.	1 div = 2.08	Th. = 65°	1 div = 2.06″	Th. = 70°	1 div = 1.78″	Th = 36°

In these observations the level was held in position in the Y's with a light spring. A second series of observations was made with the tube mounted as follows: At the suggestion of Mr. George B. Clark, oval rings of brass were fitted loosely upon the tube and held in position with wax. These rings were then placed in

Y's one end being fastened with a spring clip while the other end was free. A third series was made with the tube fastened directly to the cube of the telescope at its neutral points by means of a hard cement. The results are given below.

SERIES I.		SERIES II.		SERIES III.	
1881 May 22	1 div = 2.90"	1881 Nov. 23	1 div = 1.63"	1881 Oct. 7	1 div = 2.25"
" 22	= 2.68			" 7	= 2.25
Sept. 1	= 2.60	" 25	= 2.48	" 8	= 1.64
" 7	= 1.83			" 8	= 1.62
" 7	= 1.92	" 29	= 2.18	" 8	= 1.65
" 8	= 2.83			" 9	= 1.62
" 9	= 2.74	1882 Jan. 8	= 1.80	" 9	= 1.59
" 12	= 3.42			" 12	= 1.71
" 13	= 2.89	" 12	= 1.94	" 13	= 0.83
				" 13	= 1.28
				" 13	= 1.17
				" 20	= 1.23
Mean	2.65	Mean	2.01	Mean	1.57

It is hardly necessary to say that this level has been discarded as worthless. On the other hand the mounted level furnished with the Russian Transit proves to be an excellent one.

A similar test with a level invented by Mr. John Clark has given good results. The level tube is supported upon centres which are attached to a plate which revolves freely upon another plate described by the inventor as a "reference plate." This "reference plate" is attached to the cube of the telescope by three adjusting screws. The following are the steps of an observation.

The telescope pointing north, the bubble is read for position east and position west. The telescope pointing south, the level is now on the under side of the cube. Revolving the tube upon its centres 180°, the bubble is read as before. This level therefore gives not only the inclination of the axis, but the ellipticity of the pivots.

This form of level is especially adapted to the easy determination of the value of one division. I give below the results of the determinations thus far made. The separate results are the re-

sults of observations on different days. They therefore involve the accidental errors of observation.

												Means.
1881.	Sept.	2.78"	2.71"	2.79"	2.80"	2.96"	2.87"	2.87"	3.07"	2.63"		2.83"
	Oct.	2.61	2.99	2.79	2.53							2.73
	Nov.	2.82	2.88	3.11	3.46	2.85	2.84	2.91	2.95	2.69	2.71	2.98	2.74
	Dec.	2.85	2.96	3.13									2.98
1882.	Feb.	3.21	2.97	2.69	2.77	2.99	2.62						2.87
	Mar.	2.73	3.21										2.97
	Apr.	2.92	2.80	3.01									2.91
	May	2.85	2.93										2.89
	June	2.87	3.01										2.94

The steadiness of the level of the instrument is something remarkable as is shown by the following separate observations which are here published by the permission of Professor Pickering the Director of the Observatory.

1882	Feb.	8	$b=+.78^s$	Apr.	8	$b=+.77^s$	June 25	$b=+.74^s$
	"	5	$b=+.81$	"	14	$b=+.80$	July 3	$b=+.63$
	"	14	$b=+.82$	"	24	$b=+.81$	" 10	$b=+.72$
	"	23	$b=+.80$	May	1	$b=+.79$	" 31	$b=+.61$
	"	26	$b=+.80$	"	15	$b=+.75$	Aug. 16	$b=+.71$
	Mar.	5	$b=+.78$	"	29	$b=+.72$	" 25	$b=+.68$
	"	14	$b=+.73$	June	11	$b=+.61$	Sept. 7	$b=+.66$
	"	20	$b=+.74$	"	19	$b=+.66$	" 25	$b=+.61$
							Oct. 2	$b=+.65$

On the Arithmetic of Chords. By C. P. Hart, of Wyoming, Ohio.

[ABSTRACT.]

Ancient trigonometry, which gave birth to what is known as the "Arithmetic of Signs or Chords," though long superseded by the much more comprehensive and convenient processes and formulæ derived from the circular functions, developed by modern trigonometry, is nevertheless of considerable practical value in some trigonometrical calculations, especially when the usual logarithmic tables do not happen to be at hand. Indeed, in the solution of

certain cases of plane oblique-angled triangles, the arithmetical solution by chords is as simple and expeditious as is that of plane right-angled triangles by the Pythagorian problem, from which the arithmetical relations were originally derived.

The author, in developing these relations, discussed the following formulæ, beginning with the most simple, and obtained definite expressions for both acute and obtuse-angled plane triangles — first, for every fifteen degrees, and finally for all angles whatsoever, the latter being the equivalent of the well-known trigonometrical expression, $AC^2 = BC^2 \pm 2 \cos. B \times BC. BA.$

FORMULÆ:

θ 60°. $BC^2 = AB^2 + AC^2 - AB. AC.$

θ 120°. $BC^2 = AB^2 + AC^2 + AB. AC.$

θ 45°. $BC^2 = AB^2 + AC^2 - AB. AC \times \sqrt{2}.$

θ 135°. $BC^2 = AB^2 + AC^2 + AB. AC \times \sqrt{2}.$

θ 30°. $BC^2 = AB^2 + AC^2 - AB. AC \times \sqrt{3}.$

θ 150°. $BC^2 = AB^2 + AC^2 + AB. AC \times \sqrt{3}.$

θ 15°. $BC^2 = AB^2 + AC^2 - AB. AC \times (2 + 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 165°. $BC^2 = AB^2 + AC^2 + AB. AC \times (2 + 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 75°. $BC^2 = AB^2 + AC^2 - AB. AC \times (2 - 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 105°. $BC^2 = AB^2 + AC^2 + AB. AC \times (2 - 3^{\frac{1}{2}})^{\frac{1}{2}}.$

GENERAL FORMULA

For all oblique-angled plane triangles: $BC^2 = AB^2 + AC^2 \pm AB. AC \times BB'$ (the chord of the *difference* between the contained angle and its supplement) of a circle whose radius is one.

CIRCULAR COÖRDINATES, AND COMPLEX ANHARMONIC RATIOS. By WM. WOOLSEY JOHNSON, of Annapolis, Maryland.

[ABSTRACT.]

THE *position ratio* of a point referred to two fixed points A and B being defined as the ratio AP: BP, AP and BP being *directed lines*, is a complex quantity which may be written

$$\rho e^{i\theta} = \rho (\cos \theta + i \sin \theta) = x + iy$$

ρ and θ, or x and y, may be taken as real coördinates of P; and

it is shown that the loci of $\rho =$ constant, $\theta =$ constant, $x =$ constant, $y =$ constant are all circles. When B is removed to infinity, we have ordinary polar and rectangular coördinates. Substituting x for $1-x$ we have a symmetrical system of xy coördinates, in which it is shown that the equation of a curve is the same as that of its inverse in ordinary rectangular coördinates.

P may also be determined by the ratio of its position ratio to that of an assumed fixed point Q not on the line A B. This is a complex anharmonic ratio; when put in the form $\rho e^{i\theta} = x + iy$ the loci of $\rho =$ constant, etc., are still all circles.

The anharmonic ratio is real for four points situated on the same circle. In particular the case of harmonic ratio is considered and the general construction of harmonic conjugates is given. If two circles cut orthogonally, harmonic conjugates on one of them with respect to the points of intersection are on the same diameter of the other, and if three circles are mutually orthogonal their intersection forms three mutually conjugate pairs of points. The same construction determines points whose anharmonic ratio is i.

Inverse Elliptic Functions and the Imaginary Period. By Wm. Woolsey Johnson, of Annapolis, Md.

[ABSTRACT.]

The elliptic functions sn u cn u dn u, with their reciprocals and ratios, constitute twelve elliptic functions which, according to a notation proposed by Mr. J. W. L. Glaisher, are denoted by sn, cn, dn, ns, nc, nd, sc, sd, cs, cd, ds, dc. These may be regarded as the ratios of four quantities s, c, d, n, of which only the ratios are dealt with in homogeneous equations. The derivatives of the functions are expressed in this notation, and from them are deduced the values of the inverse functions expressed as integrals.

From the latter the usual expressions for the direct elliptic functions of an argument of the form $i\,v$ in terms of functions to the modulus k' are readily derived, and these are used to deduce the imaginary period of the elliptic functions.

CONSERVATION OF SOLAR ENERGY. By PLINY EARLE CHASE, of Haverford College, Pa.

[ABSTRACT.]

ALL forms of solar energy are due to solar radiation. The maintenance of the energy depends on the maintenance of the radiations. In investigating the relations of centripetal and centrifugal action and reaction, it seems desirable to consider the following hypotheses and conclusions:

1. Laplace's estimate that the velocity of transmission, in gravitating acceleration, if finite, is at least 100,000,000 times as great as the velocity of light.

2. Le Sage's hypothesis that gravitation and luminous radiation represent equal actions and reactions.

3. Faraday's search for a gravitating constant.

4. Herschel's comparison of the mean *vis viva* of light with that of sound.

5. Weber's identification of the velocity of light (v_λ), with the "electromagnetic ratio (v_e).

6. Berthelot's "explosive waves" and their action on waves of sound.

7. Siemens's inquiries into the combined influence of rotation, centrifugal action, gravitating fall, and chemical affinity.

To these considerations the following may be added:

8. If there is a natural unit of force, we may look for a natural unit of velocity.

9. Oscillations may be orbital, pendulous, or wave.

10. Different transformations of similar oscillations are harmonic.

11. Rotation may be regarded as a pendulous motion due to retarded and modified revolution.

12. The resemblance of Le Sage's theory to the kinetic theory of gases points to a probability that the natural unit of velocity is oscillatory. This probability is strengthened if we assume the existence of molecular and intermolecular elasticity.

13. In looking to the activities of the principal mass in our system, for indications of a natural unit of velocity, we find that gravitating velocities may be represented by gt.

14. In order that gt may be constant, t must vary inversely as g and, therefore, directly as r^2. This variation is found in the rota-

tion of a nebulous sphere, where it holds good for all stages of expansion or contraction which are not affected by external influence.

15. Gravitating acceleration should do its whole work in stellar rotation, as well as in planetary revolution.

16. Particles exposed to solar superficial gravitating acceleration during a single oscillation of half-rotation, would acquire a constant velocity which is equivalent to the velocity of light. If we designate this velocity by v_γ we have $v_\gamma = gt = v_\lambda$ as a gravitating constant, which gives the following extension to Weber's analogy; $v_\lambda = v_e = v_\gamma$. In other words, the unit of velocity which is indicated by solar rotation, is the same as is indicated by light and by electricity.

17. The velocity of light, like the velocity of sound, thus represents an elastic atmosphere whose height, if homogeneous, would be twice the virtual fall which would give the velocity in question, and whose elasticity is in harmonic accordance with solar rotation and planetary revolution.

18. Subsidence, from Laplace's limit of synchronous rotation and revolution to the poles, gives a mechanical equivalent of 76,000,000 J for each pound of subsiding matter. The spiral character of the subsidence produces solenoidal currents which may help to explain the equality of v_λ, v_e and v_γ.

CORRECTION OF A PROBLEM IN "THE SYSTEM OF THE WORLD" BY SIR ISAAC NEWTON. By DE VOLSON WOOD, of Hoboken, N. J.

[ABSTRACT.]

IT is stated "that two spheres of the same material as the earth, each one foot in diameter, placed in void space, $\frac{1}{4}$ of an inch from each other, would not come together by the force of their mutual attraction in less than a month's time."

I find that they would come together in 385 seconds, or in less than $6\frac{1}{2}$ minutes.[1]

[1] Some eminent scholars are of the opinion that the above named work was not written by Sir Isaac Newton, although his name is on the title page.

On an Experimental Solution of a Problem in the Doctrine of Chances. By T. C. Mendenhall, of Columbus, Ohio.

[ABSTRACT.]

At the meeting of the Association in 1879, the author had communicated a note upon a problem in chances attributed to Buffon, which contained the results of 12,000 trials. The problem is: If a thin stick of given length be thrown at random upon a plane surface covered with a system of equidistant parallel lines, what is the probability that it will lie across one of them? In the Proceedings of the Association for that year, a description of the apparatus and mode of making the experiment will be found.

This series of 12,000 trials was made by different individuals, and the author had resolved to carry out a more extended series alone in order to avoid a "mixing" of personal equations, if such should be found to exist. The number of trials made was 20,000. No reductions were made until the entire series was completed. At the meeting of the Association in 1881, the author had communicated the results of a reduction of about half of the total number of experiments and the results of the complete reduction were now placed before the Section. The theoretical probability involved as one of its factors π, the ratio of the circumference to the diameter of a circle. It was therefore possible to obtain a value of this constant from an experimental solution of the problem. The results of the reduction were, for convenience, put in this form showing the value of π at various stages of the experiment.

Reductions were made at the end of every one hundred throws thus giving two hundred values of this constant.

A chart was shown upon which these values were plotted together with a straight line drawn to show the true value.

The experimental curve for a time fluctuated above and below this line crossing it nine times. At the end of 7200 throws the experimental value approximates very closely to the true value. After 7900 it rises above the true value to which it does not again return. From this point until the end it gradually departs, with more or less irregularity, from the true value, and with a tendency apparently towards a constant value somewhat too great.

The final value of π is 3.180, a point which is almost constantly held during the last two thousand trials. This result seems to indicate something of the nature of a "personal equation" or bias. The only explanation for it seems to exist in the decision of doubtful cases. All cases were decided, no doubtful ones being recorded as such. The deviation from the theoretical law might be accounted for by supposing that in settling the doubtful cases there was an unconscious tendency to favor one side.

Bell Attachment for Telescope Circles. By Henry M. Parkhurst, of New York, N. Y.

[ABSTRACT.]

The bell is attached to the circles, and so arranged as to indicate when the telescope passes a designated right ascension or declination. The plan for a right ascension circle is described in the Science Observer. I have now constructed one for the declination circle, which I have here, together with photographs of the whole.

The main point is the mode of avoiding the uncertainty of position of the striking. A compound lever is so arranged that whichever way the telescope is moved, the acting portion of the lever will stand at an angle of 45°, resting upon and drawn over an arc, falling into depressions in that arc, causing the bell to sound when passing the centre of the depression, and consequently always at the same position.

The method proposed allows two strokes to come within 1° of each other, so that in sweeping in either direction the stroke will come exactly at the right point.

The Twenty-three inch Telescope of the Halsted Observatory at Princeton. By C. A. Young, of Princeton, N. J.

This instrument, which has been erected in the Halsted Observatory at Princeton within the last few months, was made by Alvan Clark & Sons, of Cambridgeport, Mass. The contract, bearing date March 3, 1880, and providing that the telescope should be completed within two years after the receipt of the disks of glass from the makers, has been carried out fully and promptly in all respects.

The disks were cast by Feil of Paris; one of them (the flint) was on exhibition at the Philadelphia centennial.

The material is unusually excellent, almost perfectly free from air-bubbles and striæ, and much more transparent than the disks (by Chance) used in the construction of the object glasses of the Washington and McCormick telescopes.

The clear aperture of the lens is twenty-three inches, and the curves are approximately as follows, according to data furnished by the makers:

Crown,	radius of	outside surface	+ 265.8 inches
	"	" inner	— 81.9 "
Flint	"	" surface next crown	— 73.4 "
	"	" " " focus	+ 222.2 "

The + sign denotes that the centre of curvature is on the side of the lens next the eye, the crown being a double convex lens, with the external surface nearly flat, while the flint is a double concave, with the flatter surface next the eye.

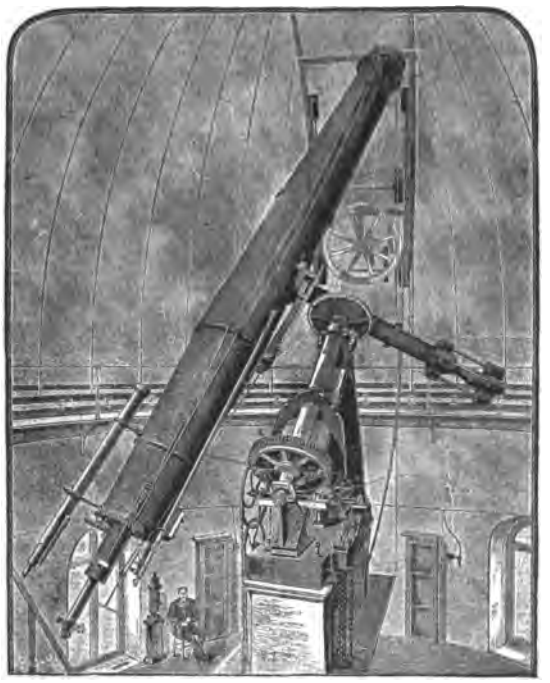
The thickness of the crown lens is 1.35 inches
" " flint " " 0.62 "
The distance between the lenses is 7.61.

The effective focus of the lens, obtained by observing star transits over a scale ruled on glass, has been found to be 361.26 inches. This is nearly a foot greater than the distance from the outside surface of the object glass to the focus, showing that the *nodepunkt* is some distance in front of the lens and not between the two glasses.

The definition is admirable, the corrections both for spherical and chromatic aberration being excellent. The "color-curve" ob-

tained by spectroscopic observation is substantially the same as that of Fraunhofer's glasses:— not quite so good as that of the Gauss object glass. The extreme range of focus between H and E amounts to just two inches.

The separation of the lenses effectually prevents the formation of any "ghosts" by reflection from the surfaces, and as the space between the lenses can easily be thrown open to the external air



THE PRINCETON TELESCOPE.

by removing the shutters which close the openings in the cell, the glass assumes the temperature of the external air very promptly. Another advantage gained is in making the aperture of the lens half an inch larger than it otherwise could be with the flint disk of given size.

There has not yet been time to test the telescope very thoroughly by actual work, but on one or two nights of good seeing, such double stars as β Delphini, and ζ Sagittæ have been satisfactorily resolved;

and Mimas, the smallest of Saturn's satellites, has been seen not only at elongation, but very near conjunction also.

The mounting presents very few new features. It is simple and solid, considerably heavier, and better planned with respect to vibrations, than that of the Washington telescope.

The tube is cigar-shaped, of steel about $\frac{1}{16}$ of an inch thick.

At the lower end a heavy brass tube, nearly six feet long and seven inches in diameter, projects about two feet, and can be moved in and out over a range of two feet by a steel screw an inch in diameter which is concealed in the lower section of the steel tube between it and the brass tube. The screw acts upon a nut attached to the brass tube, and one revolution of the hand wheel, which turns the screw, slides the brass tube $\frac{1}{10}$ of an inch. At the end of the brass tube the tail-piece proper screws on, for carrying the micrometer, or eyepieces.

The diameter of the steel tube where it is attached to the declination axis is thirty inches. From the declination axis to the object-glass extremity is sixteen feet six inches: to the lower extremity of the steel tube, ten feet nine inches, and here the diameter is twenty inches.

The total length of the right ascension axis is about nine and one-half feet. Its diameter at the lower bearing is six inches; at the upper, eight inches where it rests on friction rollers. There are two hour-circles. One, at the upper end of the axis, thirty inches in diameter, carries a coarse graduation both on the face and edge. There are three pointers, so that by means of one or other of the three, the circle can be read from any point in the room.

The fine circle, twenty-eight inches in diameter, is at the lower end of the axis, and is read to single seconds of time by two opposite verniers, which are easily accessible from a platform upon the pier.

The declination axis is nine feet long; seven inches in diameter at the bearing nearest the telescope, and five and one-half at the other bearing. The declination circle, thirty inches in diameter, is at the end of the axis next the telescope. It carries a fine graduation (to 10′ of arc) on the face next the telescope, and a coarse graduation on the edge. The coarse reading is by means of a simple pointer, and is easily made from the floor, to within 10′,— quite accurately enough to bring any object into the finder.

The fine graduation is read by means of two long microscopes

which come down to the eye end. The telescope tube carries two indices, each consisting simply of a fine line like the zero line of a vernier, and the micrometer at the eye end of the microscope gives the means of measuring the distance between this zero line and the nearest graduation mark; so that the accuracy of the reading is independent of the stability of the long microscopes. The reading is directly to 10″, and by estimation to single seconds.

The field of view of the microscope covers about $2\frac{1}{2}°$ of the graduation, and each degree is numbered. The illumination of the scale is on the principle of the collimating eyepiece, and is so excellent that the reading is very easy and accurate.

The clamps and tangent screws of both the right ascension and declination movements are managed from the eye end: the former by endless cords, the latter by Hooke's joint-handles.

There is an excellent apparatus for rapid motion in right ascension, by which the instrument can be reversed in less than half a minute.

The clockwork is very powerful, driven by a weight of about three hundred pounds with a fall of twelve feet in two hours. The regulator is on the same principle as that used by the Clarks on their chronographs, differing only in this respect that it is made very heavy (weighing over twenty pounds) and the weight is taken off the lower pivot of the vertical shaft by floating it in mercury. It runs very steadily, but seems to be somewhat affected by changes in temperature.

The instrument has two finders, one of five, the other of two inches diameter.

The tail piece of the telescope and of the five inch finder are both of the same size as that of the tail piece of the $9\frac{1}{2}$ inch equatorial of the School of Science Observatory, so that all the eyepieces and minor apparatus belonging to either instrument can be used on any one of the three.

The sliding tubes of all the tail pieces are graduated, a great convenience in setting spectroscopic and other apparatus to focus.

The instrument is provided with a filar position micrometer, fitted with Burnham's illuminating apparatus.

It has also a double image micrometer, and a square-bar micrometer for comet observations. There is a large helioscope with polarizing reflectors, and an ordinary Herschel solar-eyepiece for the finder.

A very large and powerful direct-vision spectroscope by Hilger of London has been provided for stellar work.

It is upon the same plan as the Greenwich spectroscope (except that it is a little larger), containing three so-called "half prisms," and was constructed under the kind personal superintendence of Mr. Christie, the present Astronomer Royal, who first devised this peculiar form of prism. The spectroscope collimator is 2½ inches aperture and 38 inches long. The view-telescope has the same aperture, but its focal length is much less. The instrument is over six feet long, and weighs about 150 lbs. with its appendages.

The telescope with its mounting weighs a little more than seven tons. It stands upon the top of a heavy stone pier of Quincy granite, the upper surface of which is 14 feet above the floor. The centre of motion of the telescope (at the intersection of the Right Ascension and Declination axes) is 20 feet, 6 inches above the floor.

The dome under which it is erected is of iron, 39 feet in internal diameter. It was built some fifteen years ago, and is not just what would be constructed now for the same purpose, requiring a good deal more power to turn it around, and to manage the shutter, than is the case with some built more recently:—the Vienna dome, for instance. All difficulties, however, have been overcome by placing a four-horse power gas-engine in the basement and making it do the work by means of proper gearing. It is possible to raise the shutter, turn the dome to any part of the sky, and have the telescope pointed upon any designated object within five minutes after entering the building — this includes the time required for starting the engine. The same engine drives a small Edison dynamo-machine, which supplies electric light to all parts of the building, and furnishes electric currents for spectroscopic work.

The cost of the telescope and spectroscope was $26,000, subscribed by friends of the College of New Jersey; Robert Bonner, of N. Y., R. L. Stuart, and the trustees of the John C. Green estate being the largest contributors.

On the Law of Distribution for certain Plant-Numbers.

By James Edward Oliver, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

The dimensions of any part of a plant; and also the number of flowers to a spike, of rays to a head in Compositæ, etc., when variable; have, each of them, for each species, under any given circumstances, some mean value; and I investigate, in a few typical cases, the law whereby those values of the number in question which are above or below the mean value are distributed.

I find that the number of flowers to the spike follows a law of distribution that differs but little from the normal law of distribution of errors of observation: *i. e.*, any value x has a frequency nearly proportional to the antilogarithm of $-Ax^2 \pm Bx^3 - Cx^4 \pm$ etc., wherein A, C are positive and the series convergent; as if the plant-number's deviation from the mean value were a sum of many small but sensible numbers having ambiguous signs independent of one another but dependent upon accidents of environment, ancestral history, etc.

I get a similar result concerning the *logarithm* of the length of a grass-head: indicating that the ratio of the length to a mean value is a product of many factors, each factor being a little more or less than unity as accident has determined independently of the others.

In the case of one Composita (*Leucanthemum vulgare*) I find that the distribution of the ray-numbers is strongly influenced by the "phyllotactic numbers" 13, 21, 34, . . . ; and this influence presumably combines with that of the above-mentioned law in such wise that, whenever the conditions are approximately uniform, the probability of the occurrence of a given ray-number shall differ but little from the product of the probability got as above, into a function of the ray-number's position in a cycle whose zero-points are the phyllotactic numbers. My observations, however, are too few to settle this point at present. The smaller phyllotactic numbers, 3, 5, 8, are less influential than the doubles 6, 10.

I regard the investigation as only preliminary, requiring far more varied observations to complete it; but I offer it in the hope that it may set other observers at work in the same direction.

A METHOD OF FINDING THE LAW OF LINEAR ELASTICITY IN A METAL. By JAMES EDWARD OLIVER, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

ASSUMING that the different strength of cast iron (for instance) for tension and for compression should be accompanied by a corresponding difference as to elasticity, due to the occurrence of even powers in the formula

Compressive stress $= A.\,\text{strain} + B.\,\text{strain}^2 + C.\,\text{strain}^3 + \text{etc.}$,

I get the first two or three coëfficients from experiments on the deflection of a triangular bar resting alternately on its edge and on its face.

A METHOD OF ELIMINATING THE PERSONAL EQUATION IN TRANSIT OBSERVATIONS. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

IF, during the transit, the movable wire of a micrometer could be made to move over the field at the same velocity as the star and if it could at the same time be made to bisect the latter, then a simultaneous observation of time and the position of the wire would furnish a transit observation unaffected by personal equation; we may express this otherwise by supposing the personal equation to be divided by infinity.

It would be difficult to carry this out in practice, but if, instead of dividing by infinity, we are content with a moderately large number, there would seem to be no great difficulty in thus reducing the personal equation to an inappreciable quantity.

In considering what would be necessary to effect this result there are two paths: we must either admit the usual imperfections of such apparatus and plan the mechanism in accordance therewith, or we must attempt to eliminate or control them.

An apparatus constructed in accordance with the first of these methods has already been proposed by Prof. C. A. Young, and as

it is exceedingly well contrived for giving a result free from error we will notice the main features of it. In this method the ordinary micrometer remains untouched and another horizontal slide, carrying a vertical wire, is made to move over the field by clock-work. To effect this properly a second or vertical slide is introduced (at right angles to the former) and upon it is fixed an adjustable incline, in contact with which the horizontal slide is kept by a spring. This vertical slide is driven by the clock with a velocity a certain per cent greater than that of an equatorial star, and the incline being set at an angle therewith, whose tangent equals the cosine of the declination, the vertical wire will move the same per cent faster than any other star, and will therefore creep slowly over the same. At the moment of bisection the key of the chronograph is pressed, and the current not only records the time but clamps the moving slide fast in its position, which is then determined by the ordinary micrometer. This being done the slide is released and springs up against the incline; the wire is now ahead of the star, but if the connection between the screw and the clock be made by means of a friction collar, the former can then be adjusted independently until the wire is again behind the star and ready for another observation. It is evident that this method would be affected by only the before-mentioned percentage of personal equation.

If the second method is chosen we might proceed as follows:—dispense with the ordinary micrometer and substitute therefor one of different construction. This micrometer should have a V-threaded screw as nearly perfect as possible, and it should run in two journals, one of which should be cylindrical and the other a V screw thread of zero pitch—*i. e.*, a grooved journal. The nut should be long and should embrace the screw through 90° only; it should be cut away except at its four corners, so that it would stand on the screw as by four legs. A single spring giving a thrust at right angles to the screw would now suffice to hold the nut against the screw and the latter in its journals. Such a screw would have no back-lash, and a moderate amount of wear would introduce no error in the position of the nut. The frame for holding the movable wire should be a simple extension of this nut, and would not slide in guides, the spring being so applied to the nut as to prevent its revolving about the screw axis. The screw

would carry the usual graduated head provided with an arrangement, by which its position could be registered (on a strip of paper) without stopping it. If this micrometer could be so accurately made that the reading could always be depended on for the position of the wire, this method could be made a success, and this reading could be at any time and often tested by means of the fixed wires of the reticule. A uniform motion could be supplied to the apparatus in various ways—it could be obtained from the chronograph by means of an interrupted current, made to drive a small electro-magnetic engine on the eye end of the transit. To reduce the motion for declination, a uniformly revolving sphere could be made to drive, by friction, a wheel so adjusted as to be in contact with a small circle of the same declination as the star,—a device which has already been proposed. A simpler method would be to cause one cylindrical surface to drive another by friction and to place the axis of the second at the declination angle with the first. With only the friction of the micrometer screw to overcome, either method might give good results. In order further to allow of single observations succeeding each other rapidly during a transit, the current which records the time and micrometer position should also check the latter until the star is again ahead of the wire, when the release of the key should start it again for another observation. A better way might be to have a separate key by which the motion could be altered from a certain per cent faster to about the same per cent slower than the star, so that the wire could be made to oscillate slowly over it. As the number of single observations during a transit would be at least equal to the usual number, an intercomparison might give a reliable value for the weight of the whole. The addition of an arrangement for reversing the motion when the telescope was reversed in its Ys would furnish the means of determining the error of the micrometer zero, *i. e.*, of collimation, and make this equally complete with the ordinary method.

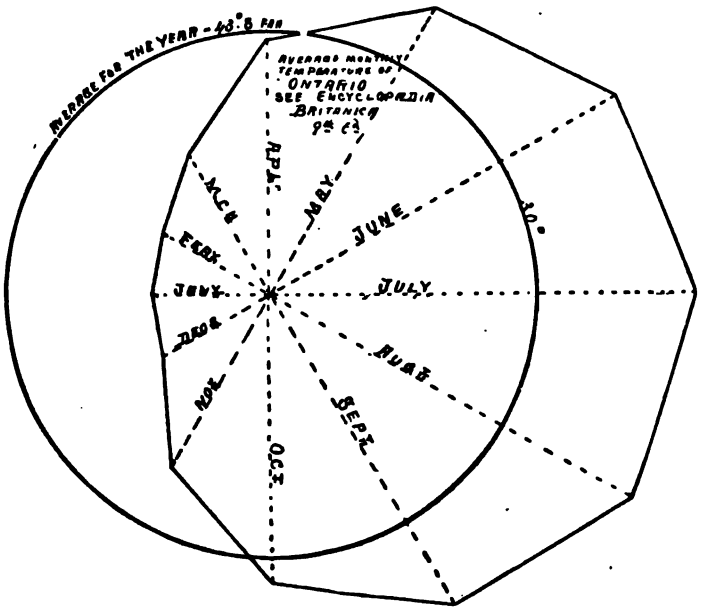
NOTE ON A NEW METHOD OF GRAPHICALLY EXPRESSING ANY CYCLICAL FACT IN METEOROLOGY.

By W. E. HAMILTON[1], of Chatham, Ontario.

(1) *Definition*:—By a cyclical fact, I mean, the fact of the continuous recurrence of certain quantities, in the same order, and separated by the same intervals of time.

(2) *Illustrations*:—The average lengths of sunlight for the successive months of the year give an illustration of quantities

FIG. 1.



Polar Coördinates. Invariable shape for any scale. Month = 30°.

which would reproduce themselves in the manner above described. Any monthly averages, deduced by statistical enquiry, such as the amounts of immigration in the consecutive months, for a given port, so far as they are assumed to be permanent and not accidental averages, would form a cycle of the kind referred to. The cycle which is graphically illustrated in the accompanying diagrams, is the averages of temperature for the months of the year, in the Province of Ontario, as given in the Encyclopedia Britannica (Fig. 1). So far as the observations go, on which the cycle is

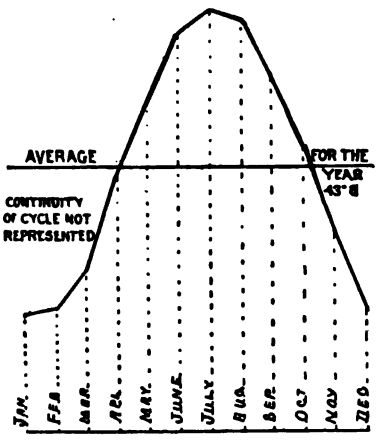
[1] Editor of the late Sir W. R. Hamilton's Elements of Quaternions.

based, it is assumed that the average monthly temperatures will recur continuously, for Ontario, from year to year, and we therefore have a cycle falling within the definition (1).

(3) *Problem:*— To express this sequence of averages graphically. It may be conceded, without argument, that no reason exists for going into three dimensions of space, and that, therefore, our diagram should be contained within a plane.

(4) *Old Solution:*—The old and usual method is to represent the element of time by an assumed arbitrary base, on which the intervals of months are represented, by arbitrary but equal spaces, separating the consecutive ordinates, proportional to the average temperatures, which are plotted on an arbitrary scale, bearing no definite relation to the arbitrary scale of months (Fig. 2). The summits of these temperature-ordinates are connected by right lines, and the unclosed rectilinear figure so constructed represents the closed and self-repeating cycle of temperatures. Temporal sequence is indicated by progression from left to right as shown by an arrow to point out the annual motion of the temperature-point. The average for the whole year is represented by a right line parallel to the base.

FIG. 2.



Ordinary method. Rectangular Coördinates. Arbitrary. Same scale as Fig. 1.

There are two objections to this method:—

(α) The visible continuity of the cycle cannot be shown, since the middle of the initial January and that of the January of the next year, are separated as the two most distant ordinates of the series.

(β) The broken line of temperatures has no invariable shape, but varies according to the ratio of the arbitrary vertical to the arbitrary horizontal scale.

(5) *New Solution:*—In my method I use polar instead of rectangular coördinates. I take a circle with an initial diameter to represent January, and successive months are separated by angles of thirty degrees from each other. On these radii, the temperatures

are plotted in their proper proportions, so that the next January temperature-radius coincides with the initial one, in length and direction. The ends of the radii are connected by right lines, and the polygon, so formed by their terminal points, represents the monthly average temperature-cycle for the year, and is visibly a cycle to the eye, while the polygon, as the simplest consideration of similar triangles will show, is of invariable shape, no matter to what scale the temperatures may be plotted. In fact, by the adoption of polar coördinates, and the consequent constancy of the angular symbol for the interval of a month, only one variable is introduced, namely, the scale of the temperature-radii. It will thus be seen that the cycle of time is patent to the eye, as a geometrical cycle of invariable shape, and that thus the objections (α) and (β) do not apply to the new method, in which, of course, the annual average is a circle with the origin of polar coördinates for centre, and a radius equal to the annual average on the assumed temperature-scale. It may be added here, that for popular illustration, the months are assumed of equal length, but that of course, in thickness, the month-angles should be proportioned to the slightly varying lengths of the months. It would be interesting did space permit, to trace the trigonometrical formulæ connecting the internal angles of the temperature-polygon, with the lengths of the temperature radii. The temperature-points might also be connected not by right lines, but by the curve of most probable curvature, which would have its evolute and other attributes of geometrical form, unchangeable in shape by alteration of the scale of temperatures. The motion of the temperature-point, in my method, is supposed to be from left to right, passing through the upper part of the figure as the spectator faces it, or in other words, the point begins to move away from the reader's body, and not toward it, when the diagram is extended before him. For monthly average temperatures in the extreme north, which may fall below zero, it is necessary, in order to avoid confusion, to take a sufficiently low temperature, say -30 for the origin of the scale of radii. In my diagrams, I assume zero of Fahrenheit, as the origin. Some minor but not utterly despicable advantages of the new method may be added. The antipode, in the annual cycle, of any given month, is pictured by a diametrically opposite point. The facts of January and July being, roughly speaking, the minimum and maximum, and the approximate equality of the fall and spring temperatures,

are patent to the scrutineer of the diagram, as is the approximate diametral opposition of the intersections of the circle of annual mean temperature with the polygon of monthly means.

(6) I submitted my method, now published for the first time, but invented more than thirteen years ago, to Prof. Kingston of Toronto, who highly approved of it, while Prof. Tait of Edinburgh thought that it was virtually anticipated by the Hodograph of my father, the late Sir W. R. Hamilton of Dublin. It is difficult in these days of general intellectual ferment to ensure priority of invention, where the energies of so many minds are converging on the same problems, and should the system have been anticipated, I can only give the right hand of greeting to my rival whoever he may be. In conclusion, I may be allowed to add that I have tried to make the explanation as full and simple as I possibly could, knowing that while a single sentence might be sufficient to give the key to the mathematical reader, there are many others deeply interested in meteorology, who would find difficulty in following a curt geometrical abstract of the system.

NOTE.—To exhibit the recurrence of the cycle, in the usual method the ordinate for January should be repeated to the right hand of the December ordinate as the representative of the mean Jan., temperature of the following year. These ordinates are replaced in my method by one and the same radius.

TITLES OF OTHER PAPERS READ IN SECTION A.

SUGGESTIONS ON THE NATURE OF COMETS, IN CONNECTION WITH RECENT ASTRONOMICAL AND ELECTRICAL DISCOVERIES. By P. H. Van der Weyde, of New York, N. Y.

SCHEME FOR OBSERVING THE GREAT ECLIPSE OF MAY, 1883. By Charles H. Rockwell, of Tarrytown, N. Y.

SECTION B.

PHYSICS.

ADDRESS

BY

T. C. MENDENHALL,

VICE PRESIDENT, SECTION B.

MEMBERS OF THE SECTION:—

NOTWITHSTANDING the many changes which have been wrought in the internal machinery of this Association by the new organization which it has put on within the past two years, and under which it now begins a sort of new life, the rule requiring an address from a Vice President has survived, and this rule will itself, I trust, render it unnecessary for me to make further apology for asking your attention briefly on this occasion.

Although this duty still lies before one who may be honored by an election to this position, yet, happily at least in the present instance, it carries with it much less responsibility under the new organization than under the old. The chairman of this section is no longer one of two, but one of nearly a half score. A Vice President is no longer expected to marshall the deeds done by half the world of Science during the past year and make them pass in orderly procession before his hearers.

We have, in fact, been shorn of even our alphabetical supremacy, an appeal to which was a last resort in friendly contests with our young and vigorous rivals of the other side. Out of respect for sciences as old as science itself we have freely accorded the first rank, as far as a designating letter may be able to imply it, to our veteran co-laborers the mathematicians and astronomers.

As re-organizers and constitution builders we may draw lines and carve out areas for which we have little respect as students of science, and no arbitrary classification can diminish the strength of the ties which bind us to Astronomy, into whose treasure-vaults we are constantly pouring rich contributions, and Mathematics, upon whose varied and wonderful resources we are obliged to draw for almost every important advance we make.

Indeed this separation of Physics from Astronomy and Mathematics as well as from Chemistry, Mechanics, etc., must be regarded as temporarily convenient for the purpose of specializing the powers of the Association.

Our sympathies, our traditions and, more than all, the constantly overlapping, intermingling currents of thought with which we are all concerned, give us the assurance that the old section, A, must continue to exist; not weaker, but stronger as a result of the digitation which has taken place, just as the fingers unite to make the hand more powerful, more cunning and more useful.

Setting out as we are upon a new and, I believe, a more efficient scheme of work, I will venture to ask the attention of this Section to a subject which may, at first, seem to many to be not quite fit and appropriate to be presented to such a body as this.

I find my justification, however, in the broad title which the general Association bears, as well as in the events of its past history. This is an Association for the Advancement of Science. The forces which have to do with aiding or retarding this advancement are so various that we are in danger of losing sight of some of them.

We are mistaken if we suppose that science is advanced only through contributions which are the result of original research in our laboratories and libraries. Even if so narrow a view be taken, it will be admitted that the talent for research is fostered and encouraged, if not, indeed, created by an atmosphere of recognition and appreciation. The existence of such an atmosphere is in itself a blessing and its production is certainly worthy of our highest efforts.

To this end it is desirable and necessary to bring about a more general diffusion of accurate knowledge concerning the elementary principles and propositions of the science of physics, as well as some degree of familiarity with the methods of physical investigation. I do not refer, of course, to the demands or the necessities

of those who expect to undergo a course of training for the purpose of becoming themselves physicists; but rather to the diffusion of this knowledge among the masses of educated people in general.

That this diffusion is not taking place to any great extent and will not, according to natural laws alone, is patent to any observing physicist, who cannot fail to have come in contact with prevailing and pernicious errors, which often carry the weight of repetition, and now and then of recognized authority.

I am aware that this is not an association of educators, and that pedagogics is not, as yet, one of the sciences specifically indicated as worthy of advancement at our hands; but if the growth of a tree is to be made healthy and permanent it is not safe to neglect the soil into which its roots penetrate. Train it and prune it as you will, to grow into vigor and strength it must spring from a rich and generous earth which, though beneath it and below it, must be in harmony with it in order to supply the proper and necessary materials for its sustenance.

It seems, therefore, not improper to raise the question, what can this Association do, or, more specifically, what can this section do to increase the efficiency of instruction in physics?

I do this the more willingly for the reason that a considerable majority of the members of the Section are engaged in this instruction during the greater portion of the year. In America a few only are privileged to devote themselves to original research. Here instruction and investigation, to a great extent, go hand in hand, and it is generally admitted that it is better so. The teacher does not reach his greatest efficiency, indeed he must fail completely unless he continues a student, not of the works of men alone but of Nature herself. On the other hand, some of the best and most fruitful inspirations of the investigator spring from his contact with those to whom he is communicating the finished products of his work. The history of science goes to show that many, perhaps most of those who have contributed to its advancement, have been great teachers. We neglect our duty, then, when we fail to give attention at proper times and under proper circumstances to the improvement of methods of instruction.

Perhaps in no other department of science has a greater change in these methods been wrought during the last ten years than in Physics. And yet this change has been going on in an irregular,

unmethodical sort of a way. There has been little or no concerted action among those interested and engaged in the work. Although all have had practically the same end in view, each individual has, in the main, worked out his own solution of the problem in accordance with his own views, modified and often largely controlled by the conditions and restrictions to which he was subjected.

It is not surprising, therefore, that results attained should in many cases be widely different and, on the whole, not entirely satisfactory.

In this new instruction many things have been attempted that could not be, and some things that ought not to be, accomplished. That over-burdened and somewhat obnoxious word *practical* has found a place in our vocabulary and we hear much of practical instruction in physics, whatever that may mean.

The subject, considered as a whole, naturally divides itself into two parts, pertaining respectively to higher instruction and elementary instruction: instruction in the colleges and instruction in the schools. Let us briefly consider each of these.

In referring to higher or collegiate instruction, it will be remembered that I do not include that of the post-graduate course in the University, properly so called, for which laboratories for research are equipped and maintained, and to which students are admitted only when thoroughly prepared by previous training. Fortunately for American students a few such courses in physics are now open in this country and it goes without saying that those who are conducting them do not need advice from us. It is for the large class of under-graduates who pursue the study of physics for a greater or less time that we may be concerned.

Contemporary with the recognition of the possibility of greatly improved methods of instruction, was the recognition of the value of the more thorough study of physics as an element in what is called a liberal education.

These were alike the results of the tremendous strides made by physical science, beginning twenty-five or thirty years ago. Grand and beautiful generalizations commanded the admiration of men skilled in other departments of human knowledge, and equally wonderful applications of principles to practice, touched our every day existence upon so many sides, as to draw forth applause from the millions who are without the "inner court." Physics thus found or forced its way upon the college curriculum to an extent

much greater than had previously been thought possible or desirable.

A few keen-sighted men, combining in themselves, happily, the student and the teacher, recognized the fact that thorough instruction in physics implied and demanded the use of laboratory methods, such as had been utilized for some years in chemistry and were rapidly coming into prominence in every other department of natural science.

Among these was, notably, Professor Pickering, whose establishment of a working, physical laboratory for purposes of instruction in the Institute of Technology at Boston, must be regarded as an epoch in the history of this progress; and with this, also, might be linked, although following at a little later date, the widely known establishment made by Professor Mayer at Hoboken.

These were quickly followed by others in the East and in the West, and at the present time there are many institutions of learning in which the laboratory methods of instruction are in use and whose equipment includes a so-called physical laboratory.

To all interested in the study of the present condition of this work I would especially recommend the very valuable Report on the Teaching of Chemistry and Physics in the United States, prepared by Professor Clarke of the University of Cincinnati, and issued about a year ago by the Bureau of Education. This Report is full of facts of great value and doubtless fairly represents the relative standing of collegiate instruction in these two important subjects at the present time.

Professor Clarke has classified the various courses of instruction in physics as follows:—

1st. Full course, including higher mathematical physics, advanced laboratory work and research.

2nd. Full course, with mathematical physics and elementary laboratory work.

3rd. Course in general physics, involving a previous knowledge of trigonometry and including laboratory work.

The other courses, up to ten in number, are elementary in their character, and do not concern the present investigation.

The report contains statistics gathered from nearly four hundred universities and colleges, agricultural colleges and scientific schools. In nearly all of these the study of physics is pursued

to a greater or less extent, although it appears that in some instances no report upon physics was forthcoming on account of ignorance as to what was meant by the word. Out of the whole number there were thirty-three institutions in which the instruction in physics fell within the limits established above. Of these there were four of the first rank, two of the second, and twenty-seven of the third.

In chemistry, however, laboratory instruction is to be found in at least one hundred and fifty institutions, the opportunities for instruction in this subject, thus outnumbering those offered for similar instruction in physics in about the ratio of five to one. But it will be remembered that in this contest chemistry has many things in its favor, and that physics is handicapped by the great cost, relatively, of the first establishment as well as by the lack of well defined and systematic courses of instruction.

Taking it as a whole it will be admitted that there has been a rapid, and, I believe, a permanent growth, and that the work has already become so extensive that it appears to be worth while to subject it to criticism and to determine by conference and consultation what improvements, if any, might be suggested.

Admitting the necessity of the laboratory as a means of instruction in physics, two important questions present themselves: first, of the total amount of time given to the subject, what proportion should be spent in the laboratory? and second, what should be the character of the work done there?

I shall not undertake to answer these questions, but will submit one or two conclusions which have been thrust upon me by observation and experience.

Concerning the first, something ought to be said. It will be remembered that the new instruction began at a time which was characterized not only by unusual scientific activity, but as well by what almost amounted to a revolution in educational processes. A great teacher had told us that we studied Nature in books, and when we met her face to face she passed unrecognized. There sprang up a new method, the essence of which was that the mysteries of nature could not be known at second-hand; that a knowledge of things could only be obtained by a contact with things themselves. The use of the text-book fell into disrepute and the student was encouraged to become his own authority. It was as

if all men were to cast aside their maps, globes, histories, books of travel, etc., and start out to obtain a knowledge of the world by visiting its different portions.

But it was soon found that many never succeeded in getting far away from home. It is true that there were those who, untrammelled by tradition, precedent or authority, made bold excursions into the regions of the unknown and returned richly laden with spoils, but these were the few; the many were found to require guidance and support for some time before they became able to carry on explorations on their own account.

The underlying principle of the new method was correct and must survive, but it was a mistake to give it universal and unrestricted application. The earlier and indeed much of the later instruction in physical laboratories was tinctured with this error.

By many we were advised that the proper course to pursue was to put into the hands of the student who, in many instances, had little or no previous knowledge of the subject, a few pieces of simple apparatus and expect him to rediscover for himself principles of physical science which although now commonplace, were at one time as completely surrounded by difficulties to the human mind as are now, for instance, the principles of the dissipation of energy and the vortex theory of atoms.

The result of the crude experiments of the student was often to disprove the law which he was expected to establish; for he lacked that knowledge and training which would enable him to take into consideration the influence of secondary causes and conditions and to determine or properly interpret the errors of experiment. Something was gained, it is true, in the way of familiarity with the methods of manipulation, but very little in the acquisition of real knowledge.

Even if this method of instruction be made reasonably successful, the actual information concerning natural laws which the student obtains must be largely superficial, often erroneous, and the rate of acquisition extremely slow. Far better would it be for him to begin his so-called practical study of the subject after becoming tolerably familiar with its general outlines and prominent features, through the study of some reliable text book and especially after having armed and equipped himself with such a training in mathematics as will enable him to discuss understandingly the results which he obtains, to consider the limitations to which they are sub-

jected and the influence which has been exerted upon them by errors of various kinds.

In this matter, as with most others, we are likely to fall into extreme views. Some of us maintain that experiment alone is the key, by the use of which Nature's mysteries are to be explored, and we fortify our belief by pointing to Faraday, the greatest experimental philosopher the world has yet produced. We forget that Faraday was ignorant only of the outward, conventional symbols of mathematical reasoning and that one of the greatest works on mathematical physics, by one of the greatest mathematical physicists of modern times is confessedly but little more than his interpretation.

Dazzled by the success of the leaders and representatives of another school, we proclaim that true success will depend on mathematical attainment, and that mathematical physics is the only physics worthy of the name. Here, again, the exceptionally brilliant few, who have succeeded under this training, stand as its exponents and we fail to consider that, if adopted to the exclusion of the first, its results may be disastrous in the extreme. No better evidence of this need be furnished than is found in the remarks recently made by Mr. G. H. Darwin, concerning a contest for honors, in what is generally admitted to be the greatest school of mathematics and mathematical Physics in existence. Mr. Darwin, who was one of the examiners, says, "The subject which exhibited the average weakness of grasp most flagrantly was thermodynamics. A great many men had read something of it, but very few really understood what they attempted to explain. Extraordinary muddle and confusion was sent up in answer to a question on the absolute scale of temperature. On another question, while the very elements of the subject were unknown to those who answered, the same men reproduced faultlessly the algebraic calculation of the thermodynamic function for a perfect gas."

Mr. Darwin also strongly recommends such a change in the style of questions as that half intelligence may be more stringently treated and men induced to read less and master more, and to gain a comprehension of physical principles.

There can be little doubt but that the experimental and mathematical study of the subject should go on together, assuming, of course, a sufficient preliminary training in pure mathematics.

What seems desirable, therefore, at least in some instances, is

less experimental work on the part of the student and more thorough and exhaustive discussion and examination of what is done.

This leads at once to the consideration of what ought to be the nature of the work done in the laboratory. The limits to which I am confined will not allow me to enter into any lengthy discussion of this important question.

I will remark, however, that in my opinion there is much done which is neither desirable nor necessary. As a rule quantitative work alone, and that the best possible under the circumstances, should occupy the time of the student. I would relegate to the lecture table of the instructor all illustrative experiments and qualitative work necessary to a good understanding of the underlying principles of the subject, which every student should possess when he enters the laboratory. That which he gets which is of most worth in his course in a physical laboratory is not a familiarity with the principles of the science, but a training in the methods of investigation in use among physicists, including a knowledge of the use and abuse of experiment and the necessary limits of our knowledge derived therefrom. The study which he ought to make of errors, instrumental and accidental, will be of great value to him in other fields than this.

As an illustration of the lack of this sort of training, I may be allowed to mention a lecture to which I listened recently, delivered by the chief engineer of one of the leading railroads in the country. The subject was the "Great Pyramid," and in speaking of certain measurements taken in the interior he declared the results, which were given in feet, inches, and thousandths of an inch, to be *absolutely accurate*, taking especial care to disclaim anything in the nature of an approximation.

I need hardly explain that he was declaiming against the introduction and adoption of a system of metrology which has done and will continue to do much to increase the simplicity and accuracy of all measurements.

I have said that this quantitative work should be of the best quality possible. It is better for the laboratory to contain a few instruments of real precision than a large number of inferior performance and accuracy. It is not a matter of great importance upon which particular department of physics a student shall spend his time and strength. The underlying principles of this method

of study are common to all and it is a matter of experience that when a student has successfully accomplished a tolerably exhaustive investigation of one topic, involving, it may be, but a single instrument with its accessories, he is upon his feet for the remainder of the course.

To sum up, the course of study in physics for the undergraduate collegian, which I have tried to indicate, should include a sufficient training in mathematics to enable him to apply his knowledge with ease and facility to the more common physical problems; a thorough and exacting course of text book and lecture work, in which the application of his mathematical knowledge would be made, and during which all illustrative experiments necessary to a complete understanding of the text should be exhibited by the instructor from the lecture table; and, finally, this to be supplemented by a course in the laboratory in which more attention is paid to the quality than to the quantity of work done; during which every problem is discussed, as far as possible, both mathematically and experimentally and especial attention is given to the discussion of the results of experiment and of the more elementary portions of the theory of errors.

Considering the work as thus divided into three parts, I am unable to see which is the least essential.

I desire to say a few words in regard to instruction in physics in the school, about which we are, apparently, more remotely concerned. Even greater reform is demanded in this direction than in the other. Although there are numerous American text-books, I venture the remark that none have properly combined, in their making, the experience of the class room with the critical knowledge of the scholar. We may import them from Europe, it is true, as we must, also, our more advanced text books, but, although in the main vastly superior to our own, they are still not entirely suited to the wants of American schools and American pupils. These books, in the hands of teachers who know little or nothing outside of the books they use and often falling far short of that, serve to put the instruction in elementary physics in this country in a condition over which no one can grow very enthusiastic; and this in spite of the prominent place which has been given it and the considerable attention it has received. Unsound doctrines and absurd theories are promulgated because of an inability to distinguish the ring of genuine metal. These become so deeply

rooted that it is difficult and often impossible in after years to clear them away.

I believe it to be possible for this Association to exert a strong influence in favor of an improvement in the character of the instruction in physics in both elementary and higher institutions of learning in America. Much of it at present does us no credit and must eventually do us much harm. At a meeting of this Association in Nashville, five years ago, a committee was appointed to report upon Science teaching in the Public Schools. At the Boston meeting, two years ago, this committee presented a report which embodied much thought upon the subject and was replete with just and keen criticisms of present systems. It is greatly to be regretted that this report cannot have found its way into the hands of those whom it would most benefit. A wide distribution ought to have been secured, and I am convinced that it is not yet too late to remedy this error.

The same gentlemen were continued as a committee to report upon the Best Method of Science Teaching in Public Schools, and it is to be hoped that a scheme may be presented at no distant day. I will venture the opinion, however, that the best results will not be obtained until this, or a similar, committee shall work in coöperation with representatives of the Public Schools themselves, and I would suggest the feasibility of securing such coöperation through the National Educational Association.

No such difficulty is in the way of securing an improvement in the teaching of physics in Colleges and Universities, for those most interested are, in the main, a part of this Association and of this Section.

I will not venture to suggest in what manner the Association might best make itself felt in this matter, although I think that would not be difficult to ascertain. I have only endeavored to direct attention to some of the salient features of the problem and to ask its consideration at the hands of many members of the Section who come in almost daily contact with it and who will, I am convinced, sustain me in the belief that it is not unworthy to be brought before this body.

In preparing this discussion of the subject, I have not had access to such information as would have been desirable, concerning the work which is being done in many institutions, nor have I

been able to consult with others who are especially engaged in its management. Indeed it was this very lack of accessible information, this very impossibility of personal consultation, with which I met in the outset, that convinced me of the importance of directing the attention of the Section to the subject.

What the Section *could* do in the direction indicated seems to be tolerably clear and certain. Whether it is wise or desirable that it should undertake to do anything is a matter which I willingly leave for it to determine.

PAPERS READ.

On the Apparent Size of Magnified Objects. By Wm. H. Brewer, of New Haven, Ct.

[ABSTRACT.]

An object seen magnified in a compound microscope by different persons impresses them very differently as to its apparent size. It is well known that such magnified object seems very much larger to some persons than to others, and a few data pertaining to this fact have been published.

Some years ago, when I had classes annually in microscopy, it was my custom to illustrate this difference of mental impression by having each member of the class in turn mark his estimate of the apparent size of some object, and when all had done, exhibit to them the estimates. This always provoked discussion, reëxaminations of the object, and wonderment on the part of some that others saw it so much larger or smaller than it appeared to themselves.

Inasmuch as I knew of no publication of specific experiments pertaining to this matter, I finally determined to keep a record of such estimates, and see what relation the average bore to the standard adopted by microscopists. Such record was begun with the estimates of my class in May, 1873. To this were added the estimates of succeeding classes. In 1876 I ceased giving instruction in microscopy, but these experiments have been continued upon such other persons as I have had opportunity to examine, until now I have a number of observations sufficiently large to give some results.

At one time or another I had used various objects for this experiment: a circular diatom, a ruled scale (as has since been used by other experimenters), an insect, and other objects; but for reasons to be mentioned later, I finally chose a small insect as best suited to my purposes.

The insect chosen was a head-louse (*Pediculus capitis*), mounted in glycerine jelly, an unusually fine specimen. I used a binocular stand, a 1½ inch objective, which, with the ocular used gave a magnifying power of about thirty-nine diameters. The estimates recorded were given in answer to a question stated somewhat thus: — "About how long, from snout to tail, does this insect *appear* to you as you see it magnified in the instrument?" If further explanation was asked, as it very often was, it was given, that the object of the inquiry might be clearly understood. Some of the observations were made with a 3-inch, a 6-inch, a 12-inch and a 24-inch rule lying on the table for convenient comparison; most of the other observations with a foot-rule similarly at hand.

The image projected with camera lucida upon a sheet of paper ten inches from the axis of the instrument, gives the following data: — length of image "from snout to tail," 4.66 inches (118.4 mm.); greatest dimensions including antennæ (or as some observers expressed it, "horns and all") 4.87 inches (123.7 mm.); diameter of field 5.85 inches (148.6 mm.). The insect was therefore entirely within the field with some room on all sides and yet extending far enough (about ⅘ across the field) so as not to appear as a minute object in a large circle of light. The instrument was always inclined at a suitable angle for comfortable observation, the object viewed by transmitted light, and nearly all the observations were by daylight.

The number of persons whose answers I have recorded is 443. About 230 of these were students in the Sheffield Scientific School of Yale College, many of whom were accustomed to use the microscope. Of the remainder, thirteen were children under twelve years of age, a few were ladies, over a hundred were mechanics and workmen, others were professional men, etc., some of whom were experts in the use of the instrument. A large house is in course of building near my own, and one day I took the microscope to where the workmen were taking their "nooning," and with the instrument set on a pile of bricks as a table, they took turns in viewing the object and making their estimates; at another time the workmen on a bridge were tried; other days I visited the machine shops near the college, and thus in a variety of ways the subjects for examination were caught.

The persons experimented upon thus embrace a great variety as to age, intelligence and vocation, from veterans in the use of the mi-

croscope to those who were looking into it for the first time; in age from less than five years to upward of eighty-five; in intelligence from eminent scientists to Irish hod-carriers and negro laborers.

Very many of the estimates were sharp and specific, as "4½ inches," "about 6 inches" or "just about 2 inches." Others were less definite, as "5 or 6 inches," "3½ or 4 inches. In such cases I have used the mean in my tables, so it must be understood that many of the estimates were not so sharply stated as the figures in the table would imply.

Of the 443 recorded numbers, two are discarded because the question was obviously misunderstood; another said the object conveyed no idea to him of size whatever, merely form, and he could give it no size. The estimates of the remaining 440, without giving the number for each fraction of an inch, may be tabulated as follows:—

Less than one inch	2
1 inch and less than 2	37
2 inches " " " 3	64
3 " " " " 4	88
4 " " " " 5	91
5 " " " " 6	56
6 " " " " 7	49
7 " " " " 8	14
8 " " " " 9	10
9 " " " " 10	7
10 " " " " 12	8
12 "	7
14 "	2
18 "	1
2 to 5 feet	4
Total number of persons	440

As the image projected at 10 inches, measures 4.66 inches, we may say that to all those whose estimates are 4½, 4⅝ and 4¾ inches, the object appeared at its theoretical magnitude. These number forty-one, about nine per cent. There were 241 estimates, or about fifty-five per cent below 4½ inches, and 158, or about thirty-six per cent above 4¾ inches. Eighty-two persons, or nearly nineteen per cent estimated from 4¼ to 5 inches inclusive, and it is fair to say

that all these saw the object at about its theoretical magnitude. There were 236, or nearly fifty-four per cent lower and only about half as many, 122 or less than twenty-eight per cent higher. There are sixty-five estimates of 2 inches or less and twenty-two of 10 inches or more; there are ten of an inch or less and fourteen of a foot or more.

This enormous difference of mental conception, as to size, immediately suggests comparisons with the impressions different people have of the size of the moon, which is really a very different matter.

The moon is seen as a bright, round, structureless object suspended against a darker sky which spreads out on every side into a vast and practically unlimited field, and the object is at an unknown but evidently considerable distance. With the object seen in the microscope, all this is reversed. We have a dark object on a bright and limited field. We look into a small dark hole through which we see the open light, and just beyond the faintly visible circling edge which limits the field, we see the magnified object. It never seems more than a very few feet distant at most. Most persons whom I have questioned on this matter say that the object appears at about the distance they would hold a book to read or any object to be plainly seen; many say less than this. It never appears at a vast and indefinite distance from us, as the moon does; it always appears in near view, as a large insect might against a circular window near, or upon a limited white surface near us. Its apparent distance from us may be, and usually is, indefinite, but it is never great.

When we see any object by natural vision so plainly and apparently so near, we always form a reasonably correct idea of its size; it is only when the distance becomes very much greater, that the judgment becomes seriously perverted and then, the greater the distance the greater the liability to such perversion.

Further, comparing this phenomenon with that of the apparent size of the moon, that is a structureless disk at an indefinite distance in a measureless field; probably if it had legs, and head, and body, or was of irregular shape like a house or even a cloud, no one would say that it looked no larger to them than a dinner-plate, as many now do. As it is, there is nothing in itself, even if it were near, to suggest any size whatever. On the other hand, the object I chose for these estimates was an organism which

when magnified was not so very unlike creatures with which we are familiar. Many compared it to a cockroach, a turtle, a lobster, or some other familiar animal. For example: two students were discussing their respective estimates which differed widely; one said earnestly to the other, after again looking into the instrument, "How is it possible that you have marked eight inches? It looks to me scarcely larger than a big cockroach." The other answered, as he too again looked into the instrument, "it seems as large to my eyes as a lobster would lying on the table before me;" and similar comparisons I have heard a score of times.

As far as I have been able to see, this mental impression as to size is reasonably definite with each individual and usually does not materially change, unless the impression at first is far from the theoretical size, and is changed later by practice in drawing with the camera-lucida. To one person the object will always appear smaller than to another. For example: a graduate student in my class estimated the length 2 inches; seven years later (during which interval he had been engaged in scientific work, and had made considerable use of the instrument) I tested him again, and his estimate was 1½ inches, he having forgotten entirely that he had ever estimated it before. Another, a professor in college who constantly uses a microscope in his professional work (to get the form of objects, but who has no occasion to draw them), estimated 9 inches; two or three years later I tested him again; he answered, "6 or 8 inches." Again, after several more years had passed, as he happened to be in my room when the instrument was on the table, I tried him again as if it was merely a casual question; he answered, "about 7 inches;" the interval between the first and last observation was more than eight years. I have kept records of duplicate estimates made by more than thirty persons, the intervals ranging from a few months to nine years, and as a rule, those who see the object as of large size first, always see it so; this is true even of children. I experimented with one of my own children, a boy in his sixth year, by having him point out on a stick how long the object appeared to him, and the length he indicated was 6 inches. Six months later I tested him again, using a very different stick and he indicated 6½ inches.

Microscopists as a class do not estimate any nearer the theoretical standard than persons unaccustomed to the use of the instrument. I have run over the list and taken the estimates of

the twenty-nine persons whom I know to be microscopists of some considerable skill, and twenty-one of them, or three-fourths of their number, estimated below the theoretical size; seventeen of their estimates were less than 3 inches and six less than 2 inches. The college professor who showed me the first good compound microscope I ever saw, now more than thirty-three years ago, and who has used the instrument more or less ever since, estimated the length at but 1¼ inches.

While the majority of microscopists underestimate the length, mechanics and artisans more often overestimate. They usually mentally divide the object into parts and from their sum make the total, and I think that their estimates average nearer the theoretical value than would those of skilled microscopists not accustomed to draw with a camera-lucida.

Regarding the few persons who estimated several feet, one was a grocer who said that the moon always seemed very much larger to him than it did to most people,—his estimate was "2 to 2½ feet. The highest estimate was by a mechanic who was accustomed to draw and plot his work. His estimate was 5 feet. The instrument stood upon a high desk in his shop, inclined highly, he examined the object leisurely and was entirely ignorant of the estimates of any one else. Upon questioning, he compared it to a picture thrown upon a screen by a stereopticon.

With most persons, if the estimate is made quickly, the impression as to size is reasonably definite, but if one stops and tries to reason upon it, it grows less so. We can illustrate this to ourselves by looking into the instrument with one eye and at a scale with the other. If the scale is fixed, the object seems of a definite length upon it, but if the scale is retreated from or approached to the eye the object covers more or less space and the impression of definite size is soon lost. One reason for choosing the object I did for this investigation was that the interest in the species, with most observers, drew attention away from conscious calculation.

Occasionally a person is found who can picture it of any size. A professor of physics, accustomed to optical instruments, says "I can see it at will of any size from three inches to three feet, but it appears best at about six inches." And I have credited him with that estimate in my list.

I do not purpose to enter here the disputed fields of psychology into which the study of visual perception naturally leads us, but

there are many instructive things brought out by these experiments. To me the most curious has been the antagonism between the impression derived from the visual angle of the magnified object, and its apparent distance from the observer. We commonly say that the impression of size is determined by the two factors, visual angle, and the distance of the object. But in these estimates it has been curious to see in how many cases the impression as to distance and size has been antagonistic. Thus a person, who had estimated the apparent size at 1¼ inches, was asked how far off from the eye it seemed, he held an object up before his eye to indicate how far off the magnified object appeared. At the distance indicated and with the visual angle actually subtended, the object should have appeared about a foot long. I have noticed many cases illustrating the same class of phenomena, where the estimates of apparent size and apparent distance were made independently of each other and without calculation, and when if one was correct, the other should have been many (even ten or more) times as large or small.

All this applies only to the mental impression of size when objects are magnified in the compound microscope. With the simple microscope there are no such great differences of impression as to magnitude. Of various other phenomena connected with these experiments, it is not the purpose of this paper to speak.

THE SPECTROSCOPIC RAIN-BAND. By WINSLOW UPTON, of Washington, D. C.

[ABSTRACT.]

CONSIDERABLE attention has been bestowed upon the variable lines in the spectrum due to atmospheric causes, especially to those due to the presence of aqueous vapor, which have been studied by Janssen, Secchi, Smyth and others. These vapor lines vary with the amount of aqueous vapor and hence can be taken as furnishing a measure of the amount present at different times. The lines near D are especially suited for this purpose and can be observed with a pocket spectroscope directed to different portions of the sky.

[The changes in the lines themselves which cause the variability of the D rain-band illustrated by chart.]

Regular observations have been made by myself with a Browning pocket spectroscope, the results of which are illustrated by a diagram giving the rain-band curve and also the amount of rainfall noted and the psychometric curve obtained from dry and wet-bulb readings.

An improvement of the instrument is suggested by introducing a shaded band for comparison.

The results obtained may be summarized:

1. The rain-band is of value in estimating the amount of aqueous vapor present in the atmosphere.

2. Its indications follow in general those of the psychrometer but have the advantage of showing the vapor present in a large portion of the atmosphere, which advantage is occasionally of use.

3. It is of some use in foretelling rain, but must be used as any hygrometer and not as an infallible indicator independent of other conditions.

4. Improvements in the instrument used are needed to increase its efficiency, before it can be regarded as a meteorological instrument.

On the Duration of Color-Impressions upon the Retina.

By E. L. Nichols, of Richmond, Ky.

[ABSTRACT.]

By a modification of the method of Plateau the author has determined for six different portions of the spectrum, the rate of revolution which must be imparted to a disk, with several narrow open sectors, in order to produce an unbroken visual image of the spectral region viewed through the disk.

The rate of revolution, which may be taken as a measure of the persistence of vision, was found to be a function of the wave length and of the intensity of the ray. The results of the experiments described may be represented by a series of curves, with wave-lengths as abscissæ and duration of the impression upon the retina as ordinates. These curves show a certain resemblance to the curves for the relative luminosity of the different spectral regions,

as measured by Frauenhofer, Vierordt and Rood. The maximum is in the same position — between D and E but nearer D than E. It seems highly probable that the duration of impression is inversely proportional to the luminosity. The curves for the duration of the visual image exhibit, it is true, relatively larger values for the more refrangible rays, but these curves correspond of necessity to a very faint spectrum. The change which the curve for luminosity would undergo, were the intensity of the ray decreased, would, owing to the greater activity of the violet nerves of the eye at low intensities, cause that curve to approach much more nearly, if it did not coincide with the curves for duration of impression.

It was also found that the curve differed greatly in the case of different observers, and to a less degree from time to time, for the same eye.

The interval of darkness which may be allowed to intervene between exposures of a given duration, without interfering with the apparent continuity of vision, and its variation when the length of exposure varied, were also determined. The exposure varied from $\frac{12}{1000}$ second, to $\frac{756}{1000}$ second, and the interval of darkness was found to vary, but in less degree.

On a mean-direction integrating Anemometer. By Chas. K. Wead, of Ann Arbor, Mich.

[ABSTRACT.]

Many forms of anemographs have been devised that record the direction of the wind at each instant as in Beckley's, or to eight points from time to time, as in the signal service record. But it is a tedious problem to obtain from these records the "mean direction" of the wind for any time by the aid of a traverse table. The instrument now described performs at each instant the resolution into an N and E component that is effected by the traverse table, and takes the sums of all similar components for any length of time.

Two equal gear wheels A and B are connected to one of the same size on the shaft of the wind-vane: on each of them a stud works in a slot in the head of the familiar yoke-headed connecting-rod; the two studs being set "at quarters," the distant end of the connecting rod will be displaced from its mean position by an amount proportional to the sine or cosine of the angle through which A and B and the vane have been turned from the meridian. On these ends friction-wheels C and D are pivoted which turn freely in a plane perpendicular to the rod. C and D slide over the faces of wheels underneath them and are driven by friction on them: these wheels, E and F, have the same velocity, and are driven either by clock-work to get a time-integration as in Coffin's tables, or by a "step by step mechanism" (as in a dial telegraph) actuated whenever a mile of wind has passed, to obtain a distance-integration. The velocity and direction in which C and D are driven will depend on their distance from the centres of E or F; that is, on the position of the studs in A and B, and so ultimately on the direction of the wind.

C and D are connected with counters or with type-wheels for printing; and the instrument is easily modified (as in the drawing exhibited), so as to find the mean direction from such old records as give the actual direction at every instant.

To find the mean direction: obviously the reading of the counter of C divided by the reading on the counter of D gives the tangent of the angle from the meridian (or the co-tangent, according to the original setting of A and B).

On Vision by the Light of the Electric Spark. By W. Le Conte Stevens, of New York, N. Y.[1]

[ABSTRACT.]

This paper is merely the continuation of an investigation, published lately in the Am. Journal of Science, in which the current theory of binocular perspective, on the principle of visual triangu-

[1] Published in full in the Am. Journal of Science for Oct., 1882; also full abstracts in the London Philosophical Magazine for Oct., 1882.

lation, has been tested in various ways. This theory still has its place in most of our text-books, being due mainly to the surviving authority of Sir David Brewster and Brücke.

The writer has shown that it fails to take into account some important physiological conditions and is wholly inadequate in many cases. He has shown that the unconscious interpretation of muscular sensation explains all that Brewster's theory explains, and much more. In studying certain mistaken observations by Brewster, he has been enabled to devise a new mode of stereoscopy, in which the perception of binocular relief, reversible at will, is secured in the examination of a pair of perfectly similar figures, by properly adjusting these in position and taking advantage of the almost spherical form of the retina in each eye. A partial explanation of this has been published, in which it was assumed that the eyes have perfect freedom of motion. Believing, however that such motion is not essential, however important it may be in completing the perception, he deemed it advisable to study this mode of stereoscopy, with visual lines parallel or divergent, by the light of the electric spark. This has been done, with the aid of Mr. W. W. Share, of Columbia College, and with apparatus loaned by Prof. O. N. Rood.

The result has been to show that the clear perception of binocular relief is attainable, not only without motion of the eyes, as first shown by Dove, but without convergence of visual lines and without the perception of double images in any part of the binocular field of view.

The Binocular Union of Spectral Images. By W. Le Conte Stevens, of New York, N. Y.[1]

[ABSTRACT.]

In studying the theory of vision the writer has had occasion to test certain experiments by Professors Wheatstone, Rogers, and Wundt, on the binocular union of images obtained by fatiguing the

[1] Published in full in the Am. Journal of Science for Nov., 1882, and London Philosophical Magazine for Oct., 1882.

two retinas with the light from illuminated diagrams, so arranged as to produce the appearance of relief, and then noting whether this appearance is recognizable in the subsequent spectral images that remain visible with more or less distinctness after the actual pictures are removed.

The source of possible error in the experiments hitherto made consists in the fact that the subsequent perception is vitiated by the observer's knowledge of what he has just seen, or of what ought to be the binocular resultant under usual conditions. This objection is applicable even in cases like that detailed by Professor Wm. B. Rogers, in which the two dissimilar diagrams were presented, each to the appropriate eye, in succession rather than at the same time. This method has been adopted in the present investigation, but with the additional important precaution, that the person tested shall not know what is the nature of the relief that should result if the two diagrams were simultaneously viewed. This was then ascertained, under varied conditions *by means of* the spectral images.

The result is a full confirmation and an extension of the conclusion reached by Professor Rogers.

These experiments, like those with the electric spark, show that muscular action is by no means necessary to the perception of binocular relief, though often an important aid; also that the perception of double images, whether consciously or unconsciously, is not necessary. The writer believes that difference in the degree of attention given to different points, at the same moment seen by direct and by indirect vision respectively, is the only indispensable condition to the perception of relief, and that the interpretation of this is a product of individual experience and not, in any demonstrable degree, of intuition. In this he accords with Helmholtz in opposition to many others who have written on this subject.

Upon the Electrical Experiments to Determine the Location of the Bullet in the Body of the late President Garfield; and upon a successful Form of Induction Balance for the Painless Detection of Metallic Masses in the Human Body.[1]

By Alexander Graham Bell, of Washington, D. C.

The subject of my present paper recalls a time of intense excitement and painful suspense. The long, weary struggle with the untimely death-wound—the prolonged suffering borne so bravely and well by the lamented President Garfield—must still be fresh in every recollection. The whole world watched by his bedside, and hopes and fears filled every passing hour. No one could venture to predict the end so long as the position of the bullet remained unknown. The bullet might become safely encysted, but, on the other hand, recovery might depend upon its extraction. The search with knife and probe among vital and sensitive tissues could not be otherwise than painful and dangerous; and the thought naturally arose that science should be able to discover some less barbarous method of exploration.

Among other ideas[2] the thought occurred that the bullet might produce some sensible effect in modifying the field of induction of a coil brought near the body of the President, and that the locality of the bullet might thus be determined without danger to the patient and without pain; for it is well known that induction can be powerfully exerted through the human body without producing any sensation whatever.

Upon the balancing of Induction.

The influence that is exercised upon induction by metallic masses has formed the subject of numerous experiments by different investigators; and the principle of balancing the effects of induction on one portion of a circuit by equal and opposite effects produced upon another portion has been utilized in nearly all such investigations.

The earliest form of induction balance for this purpose appears to have been devised in Germany by Prof. Dove,[3] about the year

[1] A preliminary notice relating to this paper was published in the *Comptes Rendus* of the French Academy of Sciences, Oct. 24, 1881.

[2] See Appendix, note 1.

[3] Pogg. Ann. vol. liv, pp. 305–335.

1841, and a good description of it in the English language may be found in De La Rive's "Treatise on Electricity," (1853 edition, vol. I, pp. 418–433) [4].

Another and superior arrangement for the same purpose is the well-known induction balance of Prof. D. E. Hughes [5].

The Static Induction Balance of J. E. H. Gordon [6] though primarily intended for experiments upon specific inductive capacity, might also, perhaps, be employed in the same class of investigations.

My own attention was directed to the balancing of induction a number of years ago by the disturbing noises produced in the telephone by the operation of telegraphic instruments upon lines running near the telephone conductor.

The difficulty was remedied by using two conductors instead of one, and by so arranging them with reference to the disturbing wires that the currents induced in one of the telephone conductors were exactly equal and opposite to those induced in the other. In this way an induction balance was produced and a quiet circuit secured for telephonic purposes. This method was patented in England in November, 1877, and during the whole winter of 1877–8 I was engaged in London upon experiments relating to the subject.

In the course of these researches I made frequent use of flat spirals of insulated wire, like those employed by the late Professor Henry [7] in his experiments upon induction.

My method was to pass a rapidly interrupted voltaic current through one flat spiral while I examined its field of induction by means of another flat spiral connected with a telephone. The currents induced in the latter coil produced a musical tone from the telephone.

At every point in the field of induction it was found that by turning the plane of the exploring coil a position of silence could be obtained, and another of maximum sound, the two positions making a right angle with one another.

It was also noticed that when a position of silence was established a piece of metal brought within the field of induction caused the

[4] A similar apparatus was independently devised in America a number of years ago by Professor Rowland, of Johns Hopkins University. It is to be regretted that his discovery of the fact that he had been anticipated by Dove prevented Professor Rowland from completing and publishing his researches.

[5] Phil. Mag., July, 1879, vol. ii, p. 50.

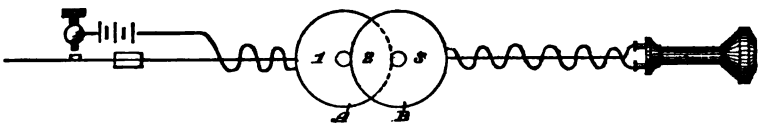
[6] Phil. Trans. for 1879, p. 417.

[7] Silliman's Journal, xxviii, 339; xxxviii, 209; xli, 117.

telephone to sound. This effect was most marked when the two flat spirals were in close proximity, and were arranged with their planes parallel, as shown in fig. 1.

When a silver coin, such as a half-crown or florin, was passed across the face of the two coils, the silence of the telephone was broken three times. The instrument emitted a musical tone when

Fig. 1.



the metallic disk passed the points marked 1, 2, and 3 in the illustration, but the loudest effect was produced when the coin crossed the area marked "2," where the two coils overlappped.

After my return to America I embodied these and other results in a paper "*Upon New Methods of Exploring the Field of Induction of Flat Spirals*," which was read before this association at the Saratoga meeting in August, 1879.

Practical Application.

While brooding over the problem of the detection of the bullet in the body of President Garfield, these experiments made in England returned vividly to my mind. It seemed to me that if the overlapping area "2" of the two coils shown in fig. 1 could be brought over the seat of the bullet without disturbing the relative positions of the coils, the telephone would probably announce the presence of the bullet by an audible sound.

A crude experiment was at once made to test the idea. A large, single-pole electro-magnet (the core of which was composed of a bundle of fine iron wires) was used in place of coil A (fig. 1;) and a small coil of fine wire taken from a hand telephone was arranged a little to one side of the pole to represent coil B. The small coil being connected with a telephone, a battery current was passed through the coil of the electro-magnet, and the battery circuit was made and broken by an assistant.

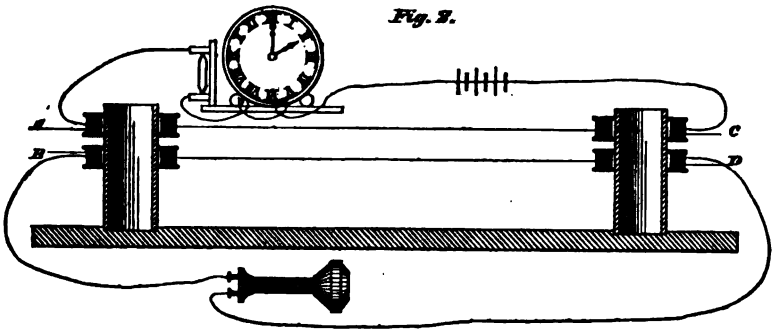
Under these circumstances a much better balance was obtained than could possibly have been anticipated. Upon now bringing a

leaden bullet near the small coil, a distinct ticking sound could be heard from the telephone each time the battery circuit was made and broken.

Being absent from my laboratory, and without facilities for proper experiment, I communicated my ideas to Mr. Charles Williams, jr., of Boston, manufacturer of electrical and telephonic apparatus, who kindly placed the resources of his large establishment at my service; and, at great personal inconvenience, delegated his best workmen to attend to my experiments.

Upon attempting to devise an appropriate form of apparatus for the special purpose in view, I saw that there were great practical difficulties in the way of utilizing the arrangement shown in fig. 1, and it occurred to me that the apparatus of Professor Hughes

Fig. 2.



might perhaps be employed with more advantage as the basis of my experiments. In the ordinary form of Hughes' induction balance four coils are used, as shown in fig. 2. Through the agency of a Hughes microphone the ticking of a clock is made to create an electrical disturbance in the voltaic circuit containing the two primary coils (A C) and a corresponding disturbance is produced by induction in the two secondary coils (B D) connected with the telephone. If the connections are so arranged that the currents induced in the telephone circuit by the coils A C are in the same direction, the ticking of the clock is heard very plainly, but if they are in opposite directions no sound is perceived.

In the latter case the action of one primary coil (A) opposes that of the other (C), and an electrical balance results. If now a piece of metal is brought near one pair of coils (say A B) the bal-

ance is disturbed and the ticking of the clock is audible at the telephone. The arrangement of the coils (A, B, C, D) was the point to be studied, the microphone attachment being of no importance in the combination; for it is well known that a rheotome to break the primary circuit completely at intervals can be substituted for the microphone with advantage.

It seemed to me that two of the coils (A B) in the Hughes induction balance might be attached rigidly to a wooden handle, so as to be moved over the seat of the bullet without changing their relative positions, and that all the adjustments necessary might be made on the other pair of coils, which need not be moved from their place, and would not therefore be liable to disarrangement. If a single pair of coils were to be used as in fig. 1, they must be adjustable one upon the other. But if during the course of exploration the coil B (fig. 1) should be moved from its proper position even to the extent only of a small fraction of a millimetre, the balance would be disturbed and the exploration might have to be stopped in order to adjust the apparatus. These considerations led me to the conclusion that some modification of the Hughes induction balance was most suitable for my purpose, and I immediately commenced the construction of such an apparatus.

Suggestions Tested.

Just at this time I learned from the newspapers that Prof. Simon Newcomb, of Washington, had the idea of using a magnetic needle to indicate by retardation of its rotation the proximity of the bullet in the body of the President, and I telegraphed to Professor Newcomb the offer of my assistance in carrying on experiments, knowing the comparative difficulty he would experience in having apparatus made in Washington.

At his suggestion I tested the point whether the rotation of a leaden disk and of a leaden bullet underneath a delicately suspended magnetic needle would cause a deflection of the needle.

The disk occasioned a deflection, but the bullet produced no sensible effect. I telegraphed the result to Professor Newcomb, and at the same time took occasion to inform him of the hopeful results I had obtained with the crudely constructed induction balance referred to above.

I was much gratified by his immediate appreciation of the ex-

periment. He telegraphed that he thought an induction balance promised a much more hopeful solution of the problem than his own method, and encouraged me in every way to continue my experiments.

This appreciation determined me to proceed to my laboratory at Washington, where I was accompanied by Mr. Sumner Tainter, who was anxious to assist in such a cause. I learned from Professor Newcomb that Mr. Geo. M. Hopkins, of Brooklyn, had independently suggested the use of Hughes' induction balance, and had made experiments in Brooklyn, the results of which were published in the New York Tribune on the 11th of July, 1881.[8] Mr. J. Stanley Brown (private secretary of President Garfield) kindly handed to me the letters he had received from Mr. Hopkins,[9] and also a Hughes induction balance like that shown in fig. 2, which Mr. Hopkins had forwarded to the Executive Mansion for trial.

This apparatus was at once tested in my laboratory, with results slightly better than those I had obtained in Boston.

My Boston apparatus did not give a greater hearing distance than 3 cm., whereas with the Hopkins apparatus I could distinguish effects at a distance of 3.75 cm.

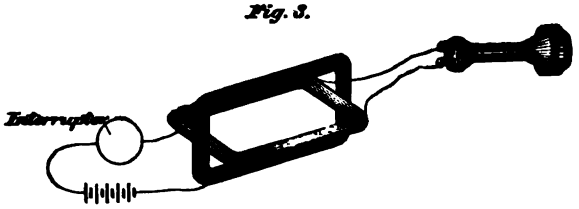
Two of Mr. Hopkins' coils (A B, fig. 2) were then fastened upon a wooden handle to form an exploring instrument, and the whole apparatus was arranged for immediate use in case of any necessity arising for an experiment upon the President. I set myself in communication with Mr. Hopkins, and requested his assistance and coöperation, and in reply received through Private Secretary Brown the following account of further experiments:

"60 Irving Place, Brooklyn, *July* 16, 1881.

Mr. J. Stanley Brown:

Dear Sir: I have made two new instruments on plans differing from that sent, but they yield no better results. The first consisted of two oblong coils arranged at right angles to each other, thus:

Fig. 3.



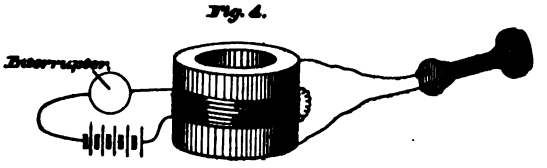
[8] See Appendix, note 2. [9] See Appendix, notes 3 and 4.

The outer coil being of coarse wire (No. 18) placed in the primary circuit, the inner coil being of very fine wire (No. 36) and connected with a telephone. The parallel currents traversing the wires neutralized each other, and no audible effects are perceived in the telephone, but on presenting a metallic body to the instrument upon a line bisecting the angle between the coils the clicking in the telephone is heard.

This instrument possesses only one advantage over that sent, and that is that it requires no adjustment.

The other instrument consists of two large coils of very fine wire (No. 36) placed upon opposite sides of a coil of coarse wire (No. 16), the fine coil being connected so that the induced currents neutralize each other, thus:

Fig. 4.



I am sorry to be obliged to say of this as of the other, that it is no more sensitive than the one sent. To produce the best effects from the instrument which you have it will be necessary to use all the battery power possible without burning the coils, and two receiving telephones of the best construction must be used.

As I stated in the first instance, if the ball is more than two inches deep, I think it cannot be located by this means.

If larger coils were used the instrument might be operative at a greater distance, but the area indicated as containing the ball would be so large that the result would be indefinite and without value.

Hoping that Prof. Bell will be able to succeed, I remain,

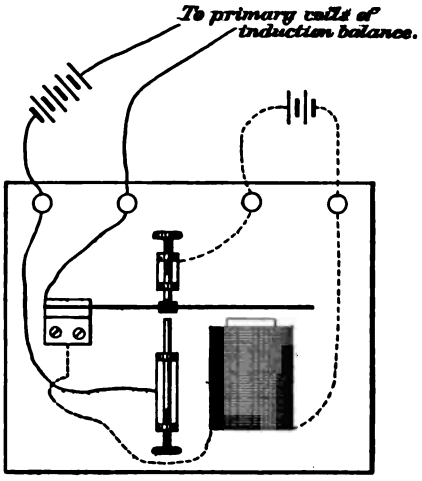
Yours very truly,

GEO. M. HOPKINS."

Prof. Hughes of London, England, Prof. Trowbridge of Harvard College, Prof. Rowland of Johns Hopkins University, and other authorities were consulted by telegraph as to the best theoretical form of induction balance for the purpose required, while empirical experiments were being carried on under my direction in my

laboratory at Washington by Mr. Sumner Tainter; in the electrical workshop of Davis and Watts, in Baltimore, by Mr. J. H. C. Watts, and in the establishment of Mr. Chas. Williams, jr., in Boston, by Mr. Thomas A. Gleason. To test the influence of size of coil an instrument was constructed in which the coils were no larger than the bullet for which we sought (as had been suggested by Prof. Newcomb),[10] and experiments were also made with the enormous coils used by the late Prof. Henry in his researches upon induction, which were kindly lent to me for the purpose by the Smithsonian Institution, but neither the small nor the large coils produced more satisfactory results than those we had already obtained.

Fig. 5.



To test battery power, twenty enormous Bunsen elements, which had formerly been used to light the gas at the Capitol, were placed at my disposal by Mr. Rogers, electrician of the Capitol, but while great electro-motive force was evidently of use we derived no advantage from such a battery as this.

To test the influence of speed of interruption, Mr. Marean, Supt. of the Western Union Telegraph Co. in Washington, kindly lent us an electric motor, by means of which we were able, with the aid of a rotating commutator, to obtain interruptions of the primary circuit of all rates up to 600 interruptions per second,[11] and we found that the more rapid the rate of interruption the more distinct was the sound in the telephone. The hearing distance, however, was not proportionately increased. The automatic interrupter (shown in fig. 5), yielding

[10] See Appendix, note 5.

[11] Mr. Sumner Tainter has since made an apparatus operating in a similar manner by means of which he has obtained as many as 4,000 interruptions of the circuit per second.

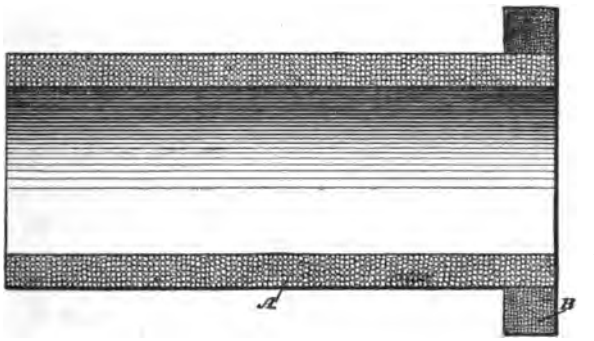
about 100 interruptions per second, gave as good results as any, and was much more convenient. This interrupter was therefore afterwards used exclusively in our experiments.

The theoretical form of coil suggested by Prof. John Trowbridge[12] was substantially the same as that proposed by Prof. Rowland,[13] and is shown in fig. 6.

The arrangement was quite sensitive to metal placed in the interior of the coil, but the hearing distance for a bullet external to the coils was no greater than before.[14]

Professor Hughes[15] proposed to have two flat superposed coils wound on a single reel, so that the two coils should form a single

Fig. 6.



one as regards their relative distance; and Mr. F. T. Bickford, Washington correspondent of the New York Tribune, suggested winding two wires side by side into a single coil, so that the relative distances of the wires from the bullet should be absolutely the same. Mr. Chas. E. Buell[16] and Dr. Chichester A. Bell[17] proposed to determine the depth of the bullet beneath the surface by causing a similar bullet to approach the balancing coils until silence was restored; the secondary bullet it was presumed would then be at the same distance from the balancing coils as the embedded bullet from the exploring coils.

[12] See Appendix, note 6. [13] See Appendix, note 7.

[14] The balance obtained was not quite perfect, and we have since discovered that the insulation of the wires of one of the secondary coils was defective.

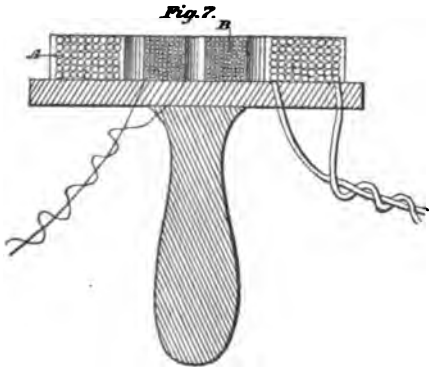
[15] See Appendix, note 8. [16] See Appendix, note 9. [17] See Appendix, note 10.

The results of all the experiments so far made were unsatisfactory. I had tried everything that had been suggested, but 4 cm. remained the extreme limit of audibility for a bullet like that which had struck the President. Even when such a bullet was flattened by being fired against a board, and was presented with its flat side towards the coils of the explorer — the most favorable mode of presentation — no better result was obtained.

Original Experiments.

In the theoretical arrangement recommended by Professors Trowbridge and Rowland (fig. 6) the primary coil A was of smaller diameter than the secondary B. This had given us no better effects than the ordinary form of Hughes' balance (see fig. 2), in which the two coils A B were of equal diameter. We then tried the effect of making the primary coil A of greater diameter than the secondary B (see fig. 7) and in this case we appeared to obtain an increase of hearing distance. Five centimetres (2 inches) was, however, the utmost limit reached, when, on the 19th of July, Mr. J. Stanley Brown and Dr. Woodward visited my laboratory and witnessed some experiments. No difficulty was experienced in detecting a bullet held in the mouth by passing the exploring coil over the cheek; and the presence of a flattened bullet held in the clenched hand was also readily determined. Dr. Bliss, Dr. Reyburn, and Surgeon-General Barnes visited the laboratory next day and expressed themselves as very hopefully impressed by the experiments. These were subsequently repeated in the surgeon's room at the Executive Mansion for the information of Dr. Frank Hamilton and Dr. Agnew, who also seemed favorably impressed.

Fig. 7.



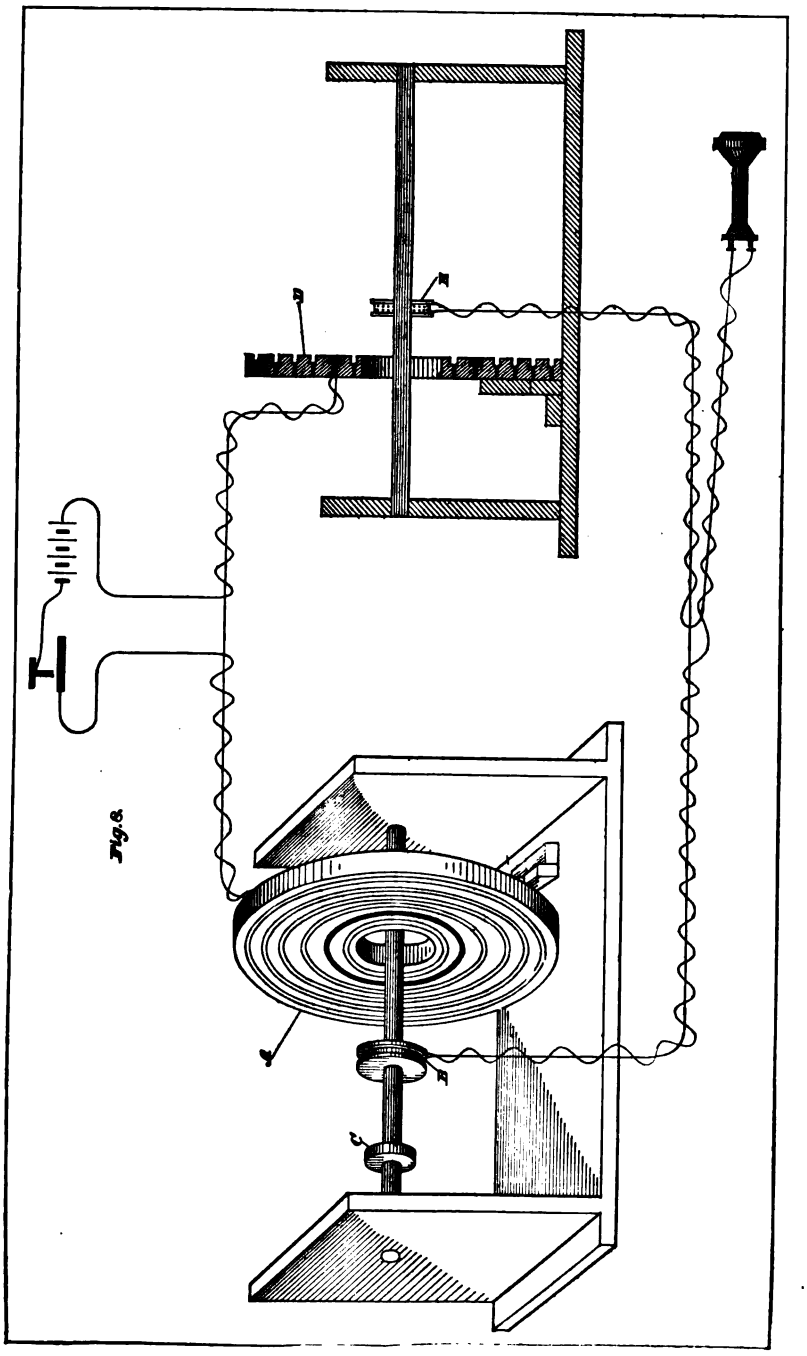


Fig. 8.

Such opinions from the surgeons in attendance upon the President, and the continued interest shown by Professor Newcomb, encouraged me to proceed with the experiments. [18]

It was now determined to test the effect of each convolution of the primary coil, so as to arrive empirically at some idea of the best shape of coil. For this purpose Mr. Tainter constructed the instruments shown in fig. 8. Circular grooves were turned in two boards, one of which is shown in perspective at A and the other in section at D. An insulated copper wire could be pressed into any of these grooves so as to give the wire an exactly circular shape of known diameter, and the two ends were passed through an orifice in the back of the board, making connection with a similar ring of wire in the other instrument as shown. A small secondary coil (B) of fine wire, which could be moved with moderate friction upon the horizontal rod, was connected to another similar coil (E) and to a telephone; and a small brass ring (C), which could also be moved along the horizontal rod, was used instead of a bullet to disturb the balance.

In making an experiment with this apparatus the secondary coil (B) was first placed within the primary ring and in the same plane with it, and the balancing coil E was adjusted to produce silence. The brass ring C was then moved along the horizontal rod until the balance was sensibly disturbed and the relative distance of the coils and the brass ring were noted.

Continuing the experiment, the coil B was moved a determined distance beyond the plane of A, and the balancing coils again adjusted to silence. The brass ring C was once more caused to disturb the balance, and the new hearing distance was noted. The following are the tabulated results of a series of experiments made on the 19th of July, 1881. The battery employed consisted of six bichromate cells connected in series.

[18] I desire especially to express my gratitude to Dr. Frank Hamilton for words of encouragement spoken at a later date when sympathy and encouragement were greatly needed.

Diameter of Primary Ring.	Distance Between—		
	A B	B C	A C
mm.	mm.	mm.	mm.
	0	17	17
	5	14	19
30	10	13	23
	20	9	29
	30	7	37
	50	0	50
	0	17	17
	5	19	24
50	10	26	36
	20	17	37
	30	14	44
	50	5	55
	0	21	21
	5	23	28
81	10	23	33
	20	18	38
	30	14	44
	50	12	62
	0	22	22
	5	25	30
113	10	27	37
	20	26	46
	30	26	56
	50	17	67

Diameter of Primary Ring.	Distance Between—		
	A B	B C	A C
mm.	mm.	mm.	mm.
	0	27	27
	5	20	25
159	10	18	28
	20	17	37
	30	14	44
	50	14	64
	0	12	12
	5	18	23
206	10	25	35
	20	19	39
	30	22	52
	50	18	68
	0	20	20
	5	18	23
253	10	20	30
	20	19	39
	30	23	53
	50	20	70

These figures show that *the distance from the primary coil* A (fig. 8), *at which the influence of the brass ring* C *became perceptible, increased with the diameter of the primary ring, and that the secondary coil* B *required to be projected considerably beyond the plane of the primary in order to obtain the maximum effect.*

The conclusion seemed a natural one that the degree of projection A B of the secondary coil should proportionally increase with the diameter of the primary ring, but the tabulated figures did not fully justify the inference.

The experiments had necessarily occupied a considerable time, and I thought that the difference between the results that should have been observed, according to the above hypothesis, and those that were actually obtained, might have been due to the gradual exhaustion of the bichromate battery employed and to its polarization, although every care had been taken to preserve its power

by removing the carbon and zinc plates from the solution, excepting when an observation was made. To test whether the battery exerted any material influence upon the hearing distance, a further series of experiments was made with the same battery.

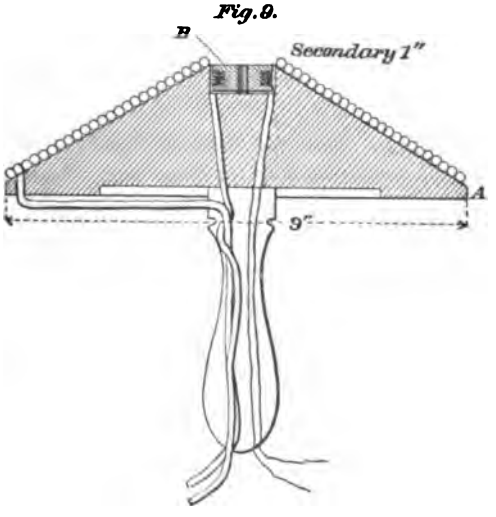
It will be seen by reference to the tabulated statement shown above that the maximum hearing distance B C had been obtained with a primary ring 11.3 cm. in diameter when the distance A B between the primary and secondary coils was one centimetre. This arrangement of the apparatus was therefore adopted throughout the following experiments:

	Hearing distance.
1. Apparatus tried with 1 cell (bichromate battery)	(B C, fig. 8) = 9 mm.
2. Six cells in series	(B C, fig. 8) = 16 mm.
3. Six cells in multiple arc	(B C, fig. 8) = 9 mm.
4. Six cells in two series of 3 each	(B C, fig. 8) = 15 mm.
5. Same experiment repeated	(B C, fig. 8) = 13.5 mm.
6. Same experiment repeated by Mr. Tainter	(B C, fig. 8) = 12.5 mm.

These experiments proved that battery power *did* exert an influence upon hearing distance, and also that the battery in use was gradually deteriorating.

I concluded, therefore, that if the battery power had remained

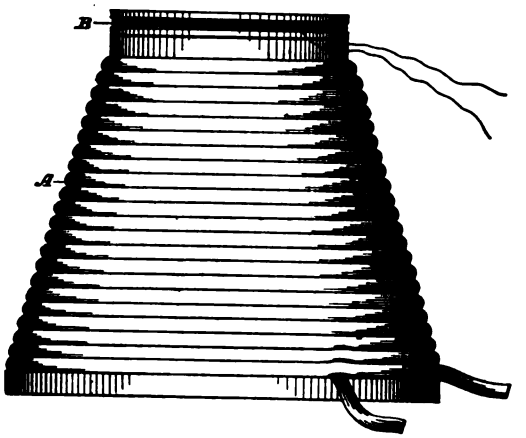
Fig. 9.



constant, the hearing distance might not only have been proportional to the diameter of the primary ring, but, in order to attain

the maximum effect, the projection of the secondary coil beyond

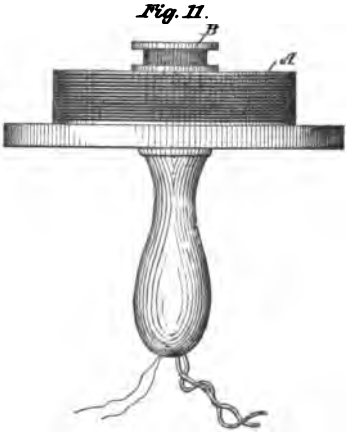
Fig. 10.



the plane of the primary might also have been found to increase in like proportion.

This led me to try the effect of a conical primary coil A with the secondary B at its apex, as shown in fig. 9, but the hearing distance for a bullet was only 3.5 cm.

Fig. 11.



Singularly enough Mr. J. H. C. Watts, in Baltimore, had independently arrived at a very similar form of coil, and with the instrument shown in fig. 10 he had obtained at one time a hearing distance of 7.5 cm. (or 3 inches[19]), but from some cause not ascertained he was unable subsequently to reproduce the effect.

The final form of apparatus adopted as the result of the above experiments is shown in fig. 11. With this arrangement and a battery of six bichromate elements freshly set up, we were always sure of a hearing distance of at least 5 cm., although after the

[19] See Appendix, note 11.

battery had been in use for some time the hearing distance hardly exceeded 4 cm.

The following are the dimensions of the coils A B (fig. 11) and their resistance:

COIL A.....External diameter . . . . . 7 cm.
Internal diameter . . . . . 4.5 cm.
Depth . . . . . 2.4 cm.
Wire used, No. 23 (cotton covered). Resistance, 2 ohms.
COIL B.....External diameter . . . . . 2.3 cm.
Internal diameter . . . . . 8 mm.
Depth . . . . . 8 mm.
Wire used, No. 36 (silk covered). Resistance, 75 ohms.

The face of the coil B projected beyond the face of coil A 4 mm.

The balancing coils were made as nearly as possible the duplicates of A and B. The resistance of the coil of the telephone employed was 75 ohms.

Influence of Battery Power.

The following experiments were made with this apparatus (fig. 11) on July 20, 1881, to test the influence of battery arrangements upon the hearing distance of a leaden bullet.

I. *Series of experiments with a bichromate battery which had previously been in use for a few minutes.*

	Hearing distance of leaden bullet as observed by—	
	A. G. Bell.	S. Tainter.
	cm.	cm.
1 cell	2.4	2.6
2 cells in series	3.3	3.5
3 cells in series	3.7	4.1
4 cells in series	3.7	4.0
5 cells in series	4.0	4.1
6 cells in series	4.3	4.4
6 cells in multiple are	2.6	2.9
6 cells in two series of 3 each — | o o / o o / o o | —	3.8	3.7
6 cells in three series of two each — | o o o / o o o | —	4.3	4.0

II. *Series of experiments with a Leclanché battery of twenty cells which had been set up for about one month. It had been kept normally upon open circuit, and had only been occasionally used.*

	Hearing distance.
20 cells in series	3.3 cm.
20 cells in 10 series of 2 each . . .	3.6 cm.
20 cells in 5 series of 4 each . . .	4.1 cm.
20 cells in 2 series of 10 each . . .	3.0 cm.

Although the battery appeared to be in good condition, a close inspection showed that the connections were dirty, and that one of the zinc wires was half broken through.

The defective cell was now removed from the circuit, and the connections of all the other cells cleaned and tightened.

III. *The following experiments were then made with the Leclanché cells united in series:*

No. of cells.	Hearing distance.
	cm.
1	2.7
2	2.8
3	3.0
4	3.3
5	3.3
6	3.5
7	3.6
8	3.8
9	4.0
10	3.8[20]

No. of cells.	Hearing distance.
	cm.
11	3.8[19]
12	4.2
13	4.2
14	4.2
15	4.3
16	4.2
17	4.2
18	4.2
19	4.2

These results are graphically represented in fig. 12.

It will be observed that the hearing distance was carried nearly two-thirds as far again as at first, simply by increasing the numbers of cells employed without any other change in the arrangement. It will also be noticed that the apparatus requires to be adjusted to complete silence in order to obtain the maximum effect.

As a general result of all our experiments with voltaic batteries, we find that *it is advisable to use a battery possessing great electromotive force and slight internal resistance, and to connect the cells in series.*

[20] Balance not quite perfect.

Hearing distance
Balance out of Adjustment.
Balance Restored.
No. of cells of battery used.

Fig. 12.

Experiments upon Living Subjects.

On the 22d of July an experiment was made at the request of Dr. Bliss upon the person of Lieut. Simpson, who had carried a bullet in his body for many years.

When the exploring instrument (fig. 11) was passed over the lieutenant's back a sonorous spot was found, but the indications were too feeble to be implicitly relied upon. Imagination very easily conjures up a feeble sound like that observed, but a number of experiments by different observers seemed to indicate that in this case there was an external cause for the sound—probably the presence of a very deeply-seated bullet. The results of this experiment were communicated to Dr. Bliss in a letter dated July 23, 1881.[21]

On the 25th of July Professor Rowland visited me at Washington, and suggested the use of a condenser in the primary circuit. I had previously discussed this idea with Mr. Tainter, but, not having a condenser at hand, we had been unable to make any experiment. After our conversation with Professor Rowland, however, we were so impressed by the importance of the point that we obtained a condenser next morning, and found it to produce not only a different quality of sound when the bullet approached the coils, but also to increase the hearing distance of the instrument shown in fig. 11 at least one centimetre.

On the evening of the same day (July 26) our apparatus was carried to the Executive Mansion, and an experiment tried upon the person of the President.[22]

From some cause then unknown a balance could not be obtained, and the results were therefore uncertain and indefinite. It was discovered afterwards that a mistake had been made in the mode of connecting the condenser. The latter should have been connected at E F (fig. 13), whereas it was placed at E G, thus influencing only one, instead of both, of the primary coils.

With the condenser properly arranged, experiments were tried on July 29 and 30 on three soldiers from the Soldiers Home who had been wounded during the civil war, namely, John Teahan, Asa Head, and John McGill. In the case of John Teahan no results were obtained. In the case of Asa Head, who had a buckshot in the cheek, loud and well-marked sounds were heard in the tele-

[21] See Appendix, note 12. [22] See Appendix, note 13.

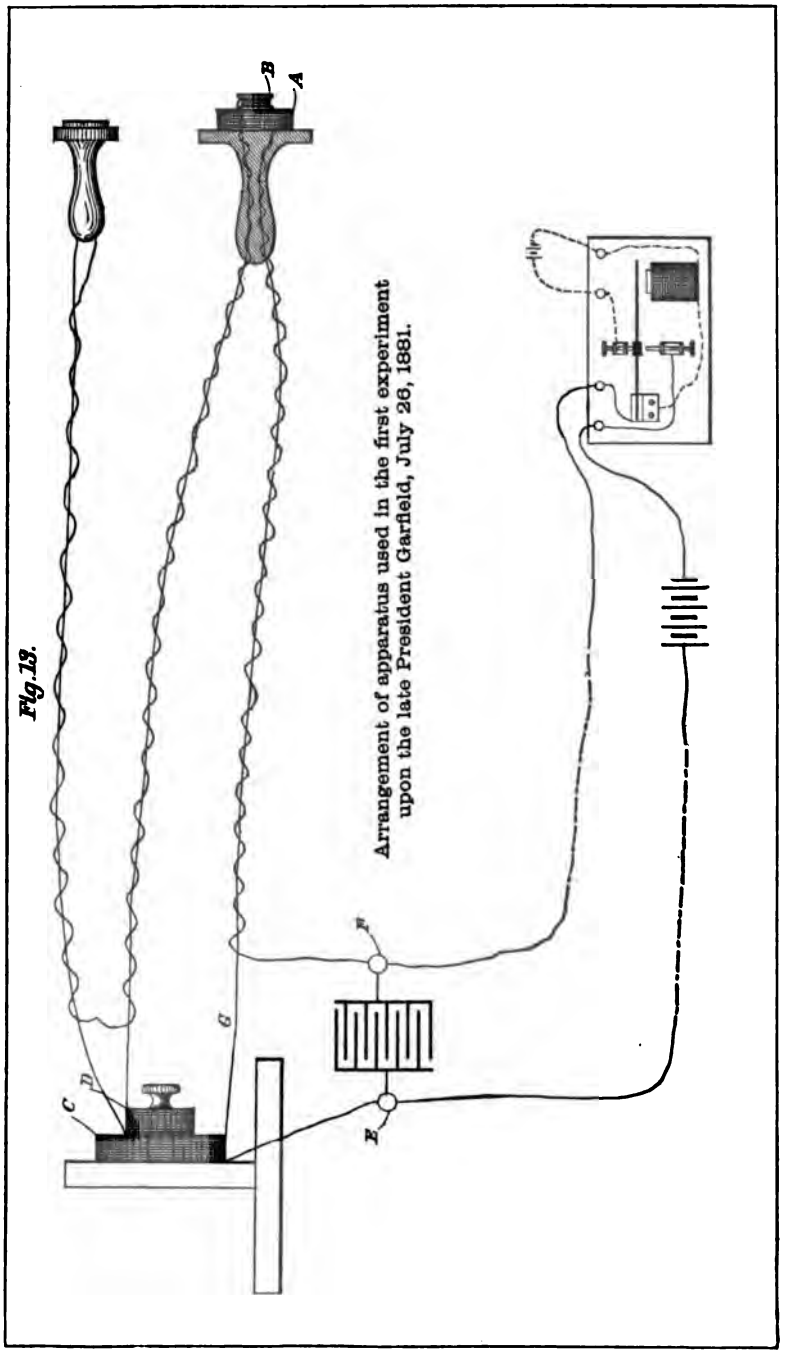


Fig 13. Arrangement of apparatus used in the first experiment upon the late President Garfield, July 26, 1881.

phone; and in the case of John McGill, who was supposed to carry a bullet in his back, no results were obtained.

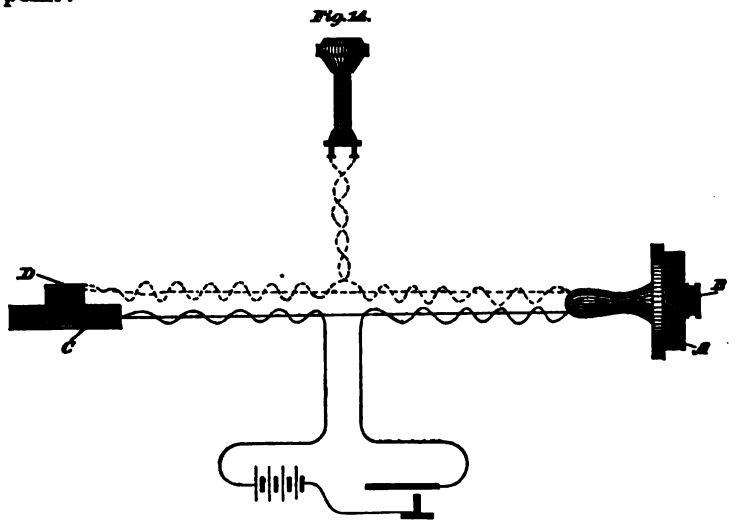
Further efforts were then prosecuted for the improvement of the apparatus.

Further Experiments to Improve Apparatus.

Our attention had hitherto been directed chiefly to modifications of the exploring instrument. We now investigated the effect upon the hearing distance, of the coils used to obtain a balance.

The following experiments, made July, 1881, bear upon the point:

Fig. 14.



EXP. 1. (See fig. 14.) Resistance of primary A of exploring instrument, 2 ohms; resistance of primary C of balancing coils, also 2 ohms; resistance of exploring secondary B, 140 ohms; and of balancing secondary D, 120 ohms.

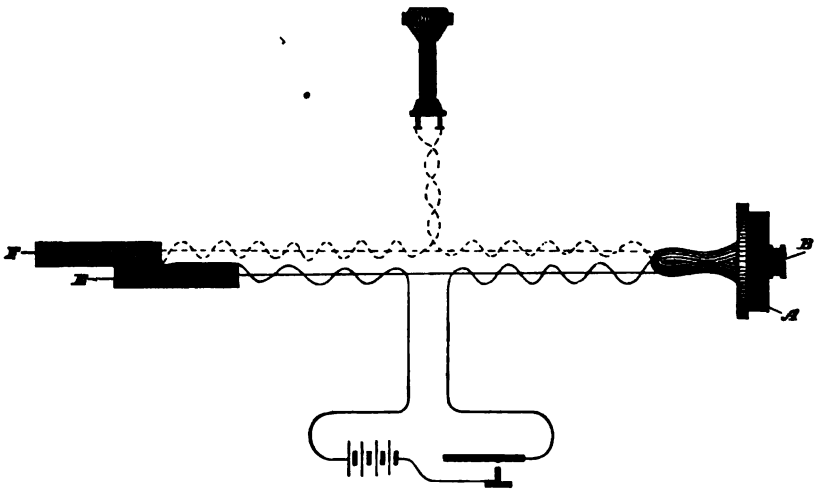
Result: Hearing distance of bullet from explorer A B, 3.5 cm. Hearing distance from balancing coils C D, also 3.5 cm.

EXP. 2. (See fig. 15.) Same exploring coils as in Exp. 1, but balancing coils consisted of a flat primary, E — resistance, 5.30 ohms; and flat secondary, F — resistance, 83 ohms. The adjustment was made by sliding the secondary coil upon the primary until a position of silence was obtained.

Result: Hearing distance from explorer A B, 1.5 cm. Hearing distance from E F, 3 cm.

As a general result of our experiments we found that *every increase in the resistance of the balancing coils (especially the primary)*

Fig. 15.



reduced the hearing distance of the exploring instrument, and it became therefore desirable to do away with this resistance as much as possible.

Return to Original Form of Apparatus.

This led us back to the original form of apparatus that had first occurred to me (see fig. 1) in which a single pair of coils was employed. A few other experiments, made July 29, 1881, will show the importance of the point attained.

Fig. 16.

Exp. 3. The two flat coils E F used in experiment 2 were arranged as in fig. 16, so as to form a balance by themselves.

Result: Hearing distance, 7 cm.

In all these experiments the battery used consisted of four cells (Leclanché).

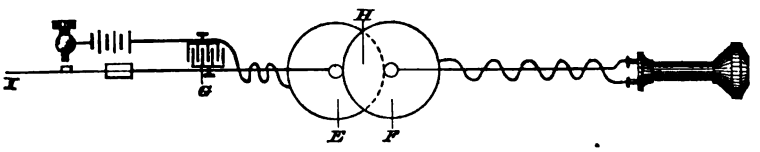
Exp. 4. The same coils used in Exp. 3 were tried again, as shown in fig. 16, but with a battery of eight cells (Leclanché).

Result: Hearing distance, 8.7 cm., or nearly $3\frac{1}{2}$ inches — a result quite unprecedented in our experiments.

The following are the dimensions of the coils E F:

Coil E.	External diameter. . . .	10 cm.
	Internal diameter. . . .	2.5 cm.
	Depth.	1 cm.
	Wire used, No. 23 (silk-covered).	
Coil F.	External diameter. . . .	10 cm.
	Internal diameter. . . .	2.5 cm.
	Depth	1 cm.
	Wire used, No. 28 (silk-covered).	

Fig. 17.



Exp. The same coils E F, used in Exps. 2, 3, and 4, were tried once more with a battery of six large bichromate elements, and with a condenser, G, in the primary circuit as shown in fig. 17.

Result: Hearing distance 13 cm., or more than 5 inches.

This great increase in hearing distance seemed to be chiefly due to the condenser, for upon disconnecting it the hearing distance was little more than 9 cm., but further experiments proved that other causes also contributed to the result.

Exp. 6. When the condenser was in circuit and the leaden bullet close to the coils (arranged as in fig. 17) the sound produced by the telephone was a musical note whose pitch was the same as that normally produced by the vibration of the reed of the interrupter. Mingled with this tone could be distinguished a number of feebler tones of very much higher pitch. Upon withdrawing the bullet gradually from the coils, the fundamental sound became fainter, and one of the high upper-partial tones gradually acquired prominence; and at a distance of about 8 or 9 cm. the funda-

mental could no longer be distinguished, but the high tone persisted, and was clearly audible up to a distance of 13 cm. The effect was very striking; and when the bullet was moved to and fro parallel to the plane of the coils E F at a distance of about 10 cm., the telephone emitted a shrill whistling sound each time the sensitive area (H) was passed.

It was noticed that other metals, such as iron, brass, and copper, did not seem to reinforce this high tone to any great extent, but brought out the fundamental at every distance where an effect was produced.

Exp. 7. The condenser G (fig. 17) was removed from the circuit and the leaden bullet held about 4 or 5 cm. from the coils E F. The fundamental tone was heard, and the characteristic upper-partial could also be distinguished, but it was only faintly audible. Upon now suddenly replacing the condenser, the high upper-partial tone was instantly reinforced as if by a resonator.

Exp. 8. The rheotome employed to interrupt the primary circuit (which had been placed in a distant room) was found to be vibrating badly. The reed I of the instrument (see also fig. 5) was rattling against its contact pieces, thus producing an impure sound, and I could distinguish amongst the upper-partials the tone that had been reinforced by the condenser. Upon screwing up the contact pieces so as to improve the vibration, I could no longer distinguish the particular upper-partial referred to, and upon returning to the room in which the coils E F (fig. 17) were placed, I could no longer detect the effects noted above in Exps. 6 and 7, and the hearing distance did not exceed 9 cm.

The peculiar effects obtained with the arrangement shown in fig. 17 *thus seemed to depend* (1) *upon a particular kind of vibration of the reed of the interrupter, producing a certain high upper-partial or overtone* (2) *upon the use of a condenser acting as a sort of electrical resonator for this tone, and* (3) *upon the use of the metal lead.*

Mr. Marean, of Washington, kindly lent me a number of condensers used by the Western Union Telegraph Co., and we found, upon connecting them with the coils E F, as shown in fig. 17, and, holding a leaden bullet near the coils, that each condenser reinforced a high upper-partial of different pitch. We arranged the condensers so that they could be successively introduced into the circuit with great rapidity. The effect was very curious, and

sounded somewhat like a Scotch air played upon the bagpipes. The low hum of the fundamental could be heard continuously, like the drone of the bagpipe, while the higher tone changed its pitch with each change of condenser.

The pitch of the high tone reinforced seemed to depend upon the electro-static capacity of the condenser employed, but the exact relation between the two has not been ascertained. In experiments 5, 6, 7, 8, and the subsequent experiments described above, the battery employed consisted of six pairs of carbon and zinc plates of large area placed in a solution of bichromate of potash containing sulphuric acid.

The effects noted above were not produced satisfactorily when the battery was much run down, nor were they obtained with a Leclanché battery which had been set up for some time, but which appeared to be in good condition.

It is evidently necessary in order to produce this characteristic high tone to use a battery possessing considerable electro-motive force and slight internal resistance.

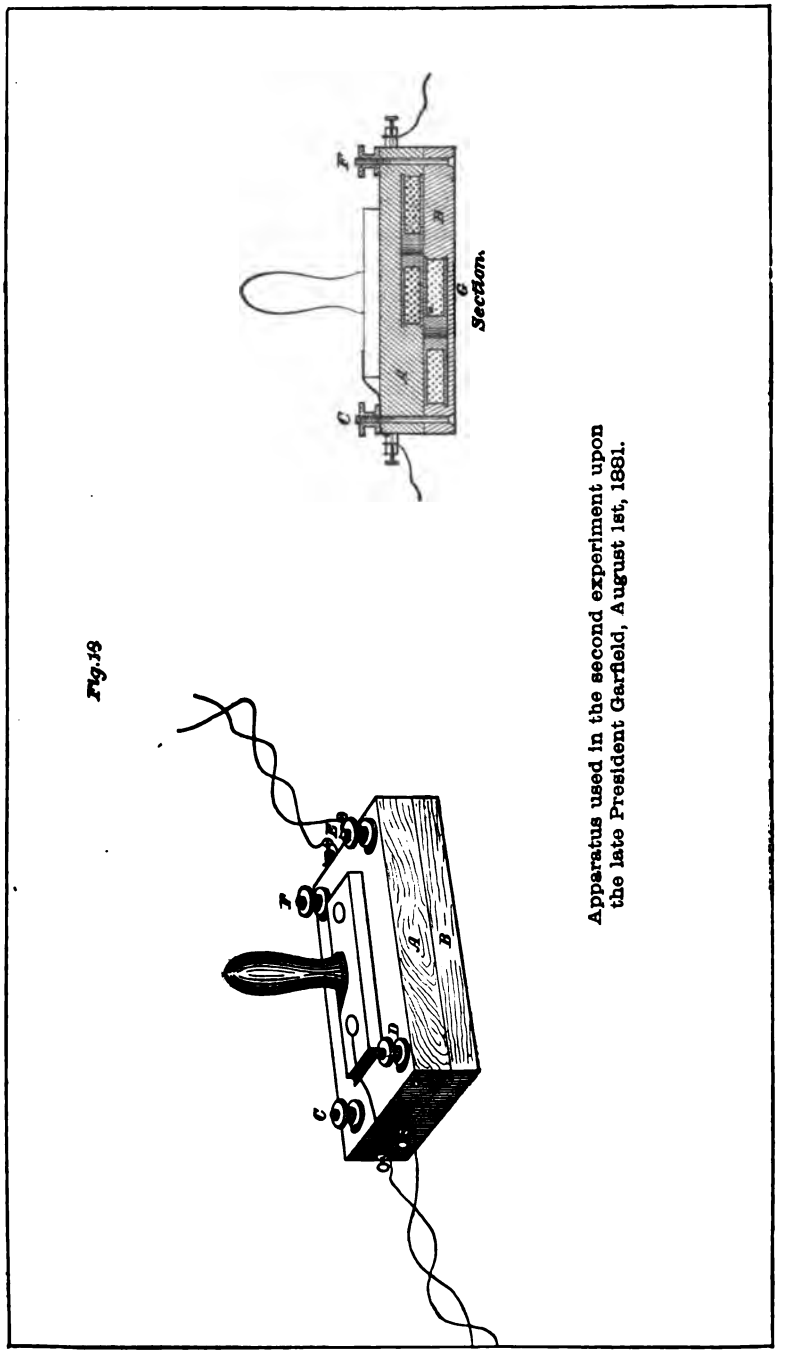
Our experiments had reached this stage when, on Saturday, the 30th of July, 1881, I was requested to make another trial upon the person of the President at the evening dressing of the wound.

At this time, however, we had no exploring instruments completed excepting one or two like that shown in fig. 11; for it will be understood that the promising results noted above had been obtained from coils that were simply placed upon a table and adjusted by hand.

We immediately proceeded to the Executive Mansion with the apparatus shown in fig. 13, prepared to make a trial, if it was deemed advisable; but upon learning of the results of our later experiments, the surgeons resolved to postpone any further trial until we could arrange the coils (fig. 17) in a portable form.

By forced exertions the coils were arranged that same night in a wooden case, as shown in fig. 18. A shallow circular recess was turned out in each block for the reception of one of the coils, and the two blocks were held together by four pins of ebonite, C, D, E, F, which passed up through slots in the upper block and were secured by ebonite thumb-screws.

When the instrument was completed I found to my great distress that a balance could not be obtained by any adjustment of the apparatus. There was a position of minimum sound, and the tel-

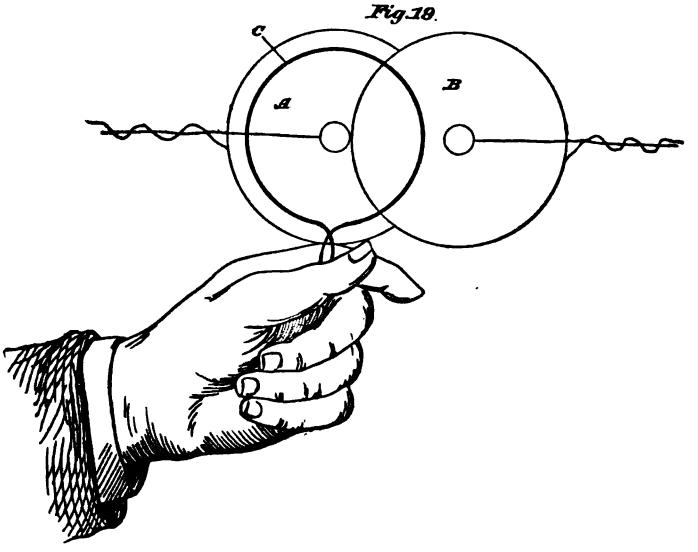


Apparatus used in the second experiment upon the late President Garfield, August 1st, 1881.

ephone responded to a bullet presented to the central part G of the instrument; but the hearing distance did not exceed 3 or 4 cm., whereas we had obtained with the same coils before the construction of the wooden case a perfect balance and a hearing distance of 13 cm.

After numerous unsuccessful experiments had been made to ascertain the cause of the difficulty it occurred to me that if two adjoining convolutions in one of the coils, made contact at any point, a circuit of low resistance would be formed (a single ring of wire, in fact), in which the induced currents might circulate

Fig. 19.



without reaching the telephone connected with the apparatus. I had previously measured the resistance of the coils without discovering any defect, but when I considered the large number of convolutions in each coil it seemed possible that a defect of this kind might exist which could not be discovered by a Wheatstone Bridge, excepting by very delicate and accurate observations. To test whether a short-circuited convolution would produce effects analogous to those observed, a piece of copper wire was bent into an annular form and the ends connected together. On bringing this metallic ring near a pair of coils (A, B, fig. 19), properly adjusted to silence, the balance was loudly disturbed. The copper ring (C) was held as shown in fig. 19, and the balance could not then

be restored by any adjustment of the coils. A position of minimum sound was all that could be obtained, and the hearing distance was enormously reduced. This was *prima facie* evidence of the nature of the defect.

The coils (fig. 18) were then removed from their case, but a cursory examination revealed no defect. Upon trial, however (being arranged, as formerly, in fig. 17), a balance could not be obtained, and the hearing distance was only about 4 cm. The defect was thus definitely located in the coils themselves.

Upon close examination it was noticed that the outside convolutions of the primary coil were slightly frayed at one part, but it appeared hardly possible that so great a defect could be due to so apparently slight a cause. However, to test the matter, I removed the outside layer of wires and then tested the coils again.

Result: The defect had vanished — a perfect balance was obtained, and the hearing distance was again 13 cm. [23]

The coils were then replaced in their case and the completed instrument tested. The lower wooden block B (fig. 18) was adjusted by hand as nearly as possible to the position of silence, and then the thumb-screws C, D, E, F were tightened.

The balance now obtained was not quite perfect, but by striking

[23] These experiments have revealed the cause of the extreme difficulty always experienced in obtaining a perfect balance with coils of fine wire. I have recently used an Induction Balance to test the condition of the helices that were employed in these researches, and have discovered that in a large percentage of cases the insulation was defective. It is possible that some of the results described in this paper (especially of the earlier experiments) may have been vitiated by errors due to defects in the coils that were not suspected at the time. A defect of insulation that is quite immaterial for ordinary purposes may be absolutely fatal to the success of an Induction Balance. Indeed, so much care is required in this respect that it is extremely difficult to obtain coils that are perfectly suitable for an apparatus intended to search out a bullet embedded in the body. I now make it a rule to test every helix used in Induction Balance experiments by bringing it up to a system of balanced coils like that shown in fig. 19.

1. If the helix is perfect the balance is not disturbed until the terminals of the coil are connected.

2. If there is a break in any of the convolutions the balance is not disturbed, even when the terminals are connected.

3. If a convolution is short-circuited the balance is disturbed, even though the terminals are not connected, and the sound produced is the fundamental of the rheotome employed to interrupt the primary circuit.

4. If the insulation is defective the balance is disturbed, although the terminals are not connected, and a peculiar spluttering effect is noticed like that produced by a series of sparks.

I propose to apply this method practically as a means of testing the condition of the helices used in the construction of Induction Coils and those employed in the manufacture of telephones.

the lower block B a few smart blows with a wooden mallet we were able to reduce the arrangement to complete silence.

The instrument was then in such a sensitive condition that it could scarcely be moved without affecting the balance. Upon gently swaying it backwards and forwards a pulsation of sound was heard at every swing.

When the motion was carefully made, so that it was always in the same plane, no pulsations were observed. They only occurred when the inclination of the coils was changed.

This defect was found to be due to the bulging of the thin portion G of the wooden case (fig. 18) under the weight of the enclosed coil, and the simple pressure of a finger on this portion of the case disturbed the balance. The movement of the lower coil when the instrument was swayed about must have been inconceivably small, but on account of the extreme sensitiveness of the arrangement it produced a perceptible effect upon the balance.

The pulsating sound did not seem to interfere with the detection of a bullet held in the clenched hand, nor did it seem to affect the hearing distance. I therefore despatched a messenger to the Executive Mansion (Sunday morning, July 31) with a note for Dr. Bliss,[24] to let him know that the instrument was in a condition to be used, should any necessity arise for an immediate experiment. At the same time I informed him that the apparatus in its present form was very crudely constructed, and that I hoped to improve it very greatly in the course of a few days. On Sunday afternoon (July 31) we sent to the Soldiers Home for John McGill, upon whom we had experimented the previous day without results (using the apparatus shown in fig. 11).

Upon trying the new instrument (fig. 18) we had no difficulty in finding a sonorous spot in his back, at the place where the bullet was always supposed to be.

This result was at once communicated to Dr. Bliss,[25] and in reply we were requested to make the experiment upon the person of the President next morning.

On Monday morning (August 1, 1881) we accordingly removed our apparatus to the Executive Mansion.

[24] See Appendix, note 14. [25] See Appendix, note 15.

The Late President Garfield.

During the former experiment (July 26) a sudden sonorous effect had been observed upon passing a point near the spot where the surgeons suspected the bullet to be lodged, but I had been unable to verify this by a second observation, although the exploring instrument (A B, fig. 13) was repeatedly passed over the same place. The sound had been so loud and well marked that I believed at the time it must have been caused by a sudden irregularity in the vibration of the reed of the rheotome used to interrupt the primary circuit, for the arrangement (as explained above,) was not perfectly balanced, and any irregularity of this kind would, under these circumstances, have affected the telephone. At the same time the coincidence was remarkable that the exploring instrument should have been at that very time so near the suspected seat of the ball, and this led to the thought that perhaps after all the bullet had been the cause of the sound. I felt confident that the new instrument (fig. 18) would at once decide the question, for the extreme hearing distance of the former apparatus (fig. 13) was only 6 cm., and the apparatus shown in fig. 18 was so superior in this respect that if the sound had really been due to the bullet we should obtain with the new instrument distinct and well-marked effects. When the new explorer (fig. 18) was passed over the suspected spot nothing was heard excepting a slight pulsating sound as the instrument was moved to and fro. This was evidence to me that the former sound had been of accidental origin, whether the bullet was there or not. With a view of eliminating any error of observation caused by the pulsations due simply to the movement of the instrument, I lifted the latter (without changing the inclination of the coils) to a height of about fifty centimetres above the body of the President, and moved it to and fro in as nearly as possible the same way I had done at the lower elevation.

I presumed that if the pulsations heard were due simply to the movement of the instrument, they should occur with equal strength at the two elevations; but if any portion of the sonorous effect was due to the influence of the bullet, the pulsations at the two elevations would be different in intensity. I was struck by the fact that, although the sonorous pulsations were very feeble, they were sensibly louder when the instrument was close to the surface of the body than when it was raised. Continuing the exploration, I

found a considerable area over which similar effects were noticed, but upon carrying the instrument towards the back of the President, the difference between the pulsations produced at the two elevations grew less and less, and finally could not be distinguished.

The difference in the loudness of the sound at the two elevations was so slight that it probably would not have been noticed by an ear unaccustomed to listen to feeble effects, and I feared that the general expectation that the bullet would be found in that part of the body might have led me to imagine a difference that did not exist. For the purpose of eliminating as far as possible any personal error, I requested Mr. Sumner Tainter (who was the only other person present whose ear had been sufficiently trained to be reliable in such an emergency) to repeat the experiments and let me know the result. Upon our return to my laboratory we compared notes, and I found that his observations tallied with mine. He declared he could not obtain a distinctly localized effect, but stated that he had observed a reinforcement of the pulsation over an area of at least two inches in the neighborhood of the spot to which his attention had primarily been directed, and that he was convinced that the bullet was within that area.

It appeared reasonably certain that the area of feeble sound was due to some external cause, and was not simply an effect of expectancy. In the absence of any other apparent cause for the phenomenon I was forced to agree in the conclusion that it was due to the presence of the bullet, and I so stated in my report to the surgeons.[26] I was by no means satisfied, however, with the results obtained, for no such effects had been observed before in our experiments with bullets. I tried to reproduce the effects by moving the instrument (fig. 18) at different distances over a bullet, but in every case where an effect was produced the sound was quite sharply localized. I thought that perhaps the body of the patient might have affected the result, and so experimented upon a bullet buried in a piece of meat, but no difference of effect was noted. This led me to fear that the extensive area of feeble sound might have been due to some extensive area of metal that was unsuspected at the time, and I proceeded to the Executive Mansion next morning (August 2) to ascertain from the surgeons whether they were perfectly sure that all metal had been removed from the

[26] See Appendix, note 16.

neighborhood of the bed. It was then recollected that underneath the horse-hair mattress on which the President lay was another mattress composed of steel wires.

Upon obtaining a duplicate, the mattress was found to consist of a sort of net of woven steel wires, with large meshes. The extent of the sonorous area having been so small, as compared with the area of the bed, it seemed reasonable to conclude that the steel mattress had produced no detrimental effect.[27] I was unable to continue experiments with the steel mattress, as just at this time I was obliged to leave Washington on account of illness in my family. Although I was unable for a long time afterwards to carry on personally Induction Balance experiments, the investigations were ably continued under my direction by Mr. Thomas Gleason, in the establishment of Mr. Charles Williams, Jr., in Boston.

Experiments continued in Boston.

Mr. Tainter forwarded from Washington drawings of an improved apparatus he had designed, to remedy the defects of the instrument shown in fig. 18, in which the case, adjusting screws, etc., were all to be composed of ebonite.

Mr. Gleason constructed for me a number of such ebonite instruments differing slightly from one another in detail, and the apparatus shown in fig. 20 combined the different points that had been approved.

The two coils A B were eccentrically arranged in two circular disks of ebonite, C D, and the adjustment was obtained by means of an ebonite key O, like the key used for tuning pianos, which turned a cam M pivoted in the upper disk and working in a slot N in the lower disk.

In order to prevent any movement of the coils, excepting that produced by the adjusting-key O, each coil was placed in a recess turned out in its ebonite disk, the edges of which were bevelled as shown at R. Paraffine was then poured in so as to fill up each recess. But this alone did not prevent a slight pulsation of sound when the instrument was swayed from side to side, and a very slight pressure of the finger on the thin portion of the ebonite plate under the coil B was sufficient to destroy the balance.

[27] The death of President Garfield and the subsequent *post-mortem* examination, however, proved that the bullet was at too great a distance from the surface to have affected our apparatus.

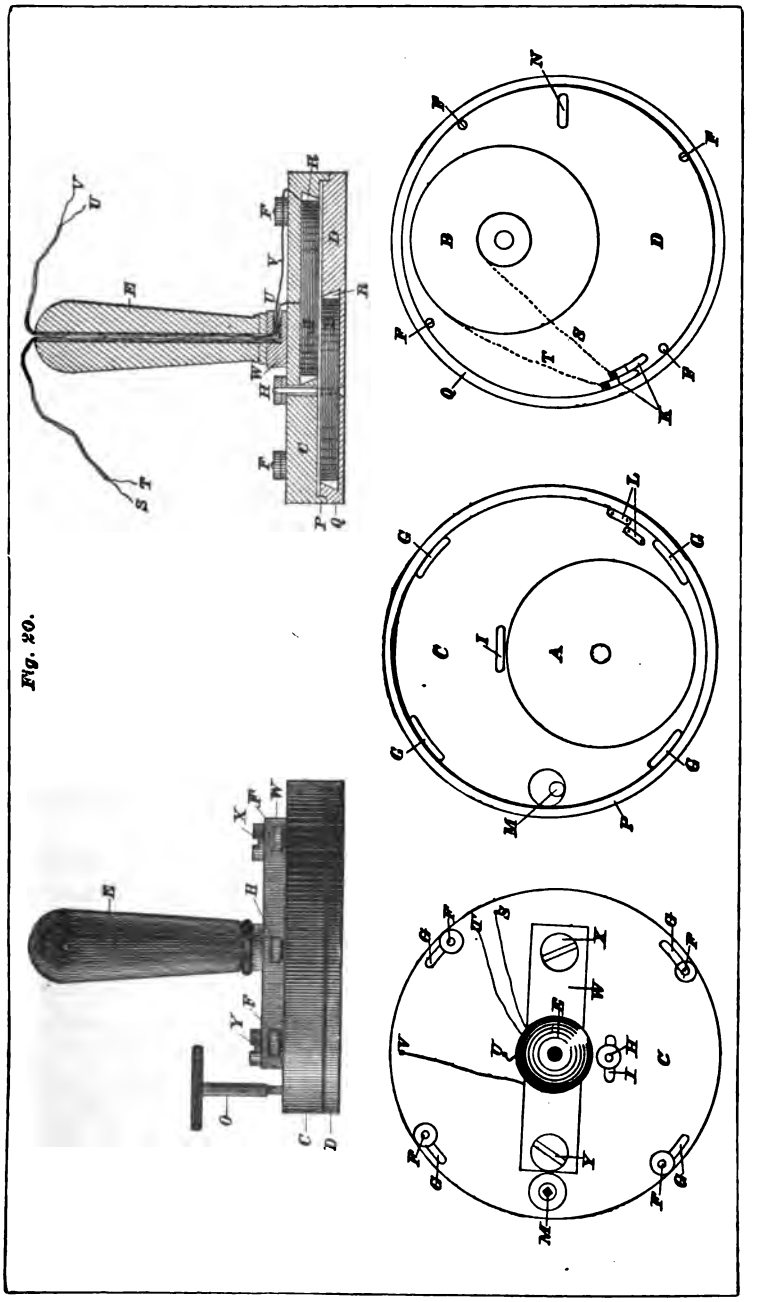
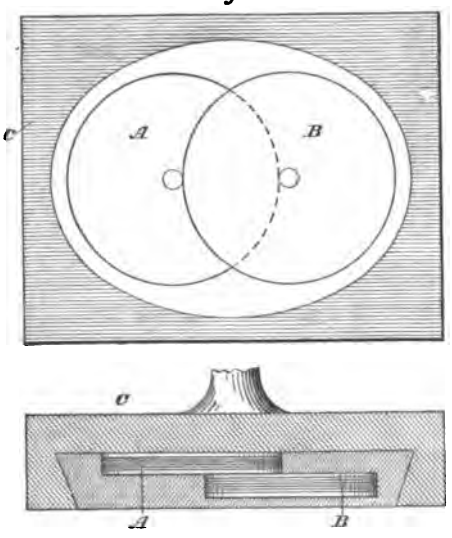


Fig. 20.

This was remedied by strengthening this portion by means of a rod of ebonite, which passed up through the centre of the coil and through a slot I, in the upper ebonite plate, and was clamped firmly after the adjustment of the instrument by an ebonite thumb-screw H. This, however, increased the difficulties of adjustment. When the coils were adjusted to silence, then the tightening of the thumb-screw H disturbed the balance; and if the thumb-screw H was tightened first, then the adjustment could only be made by a series of jerks, on account of friction. In practice we found it best to adjust the instrument *almost to silence*, and then the tightening of the thumb-screw H completed the balance.

Fig. 21.



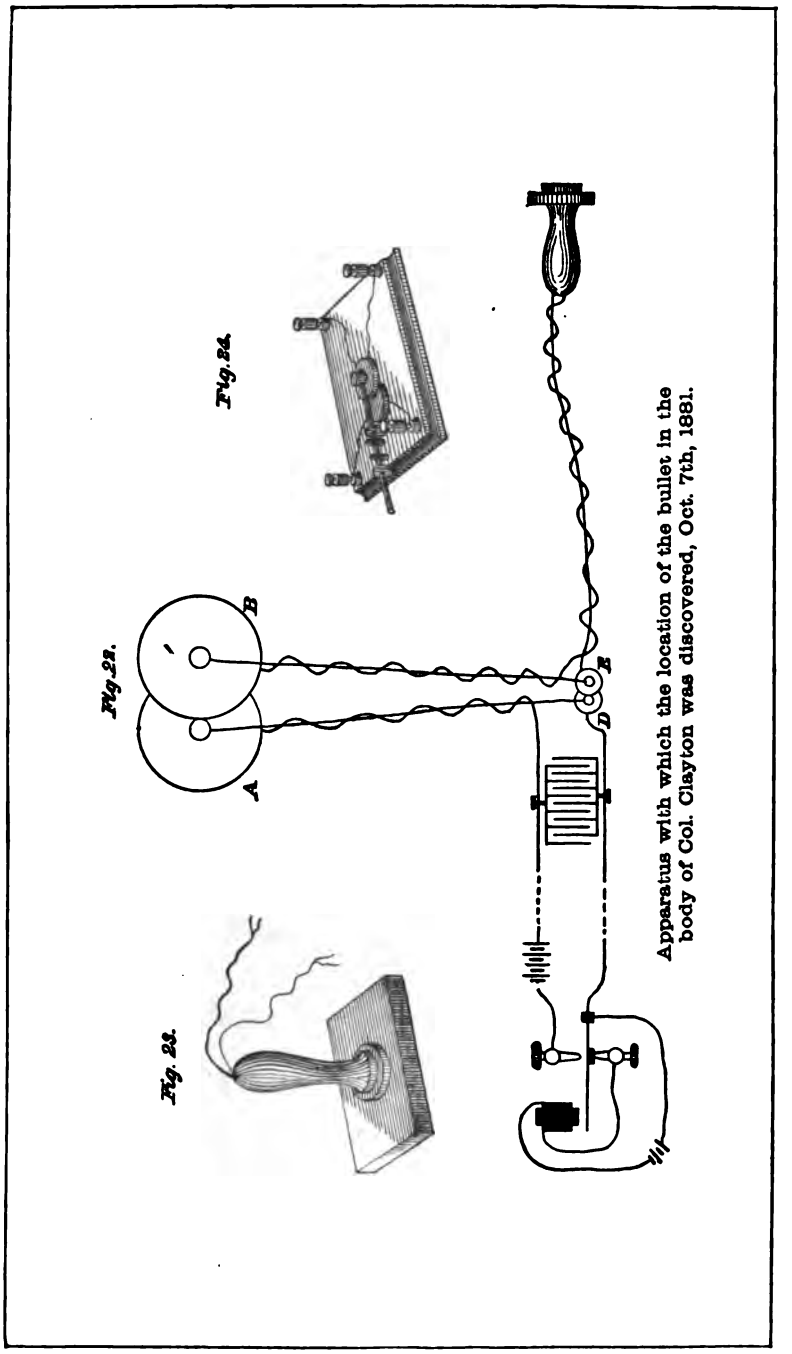
This was the form of apparatus at which we had arrived at the time of the death of President Garfield.

The difficulty of adjusting the coils led me ultimately to the idea of the apparatus shown in figs. 21, 22, 23, 24, which is the most practical form of the instrument yet devised.

The two exploring coils A B (fig. 21) are arranged as shown, in a recess turned out in a single block of wood C.

The coils are temporarily connected with a telephone, battery and rheotome in the manner shown in fig. 1, so that they may be adjusted by hand to form a balance. When they have been arranged in their position of silence the hollow in the block of wood C (fig. 21) is filled with melted paraffine. Upon cooling, the two coils are found immovably fixed in one solid cake of paraffine.

As a matter of practice it is found impossible to fix the coils in this way exactly in their position of silence; but by means of two other very small coils, D E (fig. 22), of insignificant resistance, forming a sort of fine adjustment external to the explorer, a per-



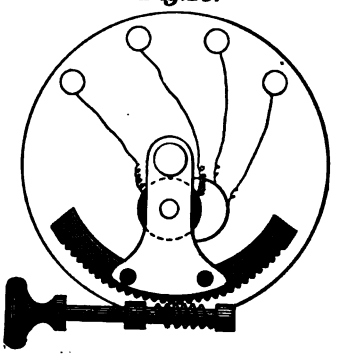
Apparatus with which the location of the bullet in the body of Col. Clayton was discovered, Oct. 7th, 1881.

fect balance is easily obtained. In this instrument the swaying of the coils A B produces no effect upon the balance.

The completed arrangement is shown in plan in fig. 22, and the explorer and balancing coils are shown separately in perspective in figs. 23 and 24.

On account of the small size and slight resistance of the balancing coils we were enabled to make the adjustable parts of the balancer of metal without practical interference with the sensitiveness of the exploring instrument, and this gave us the power of making very delicate adjustments of the balancing coils.

Fig. 25.



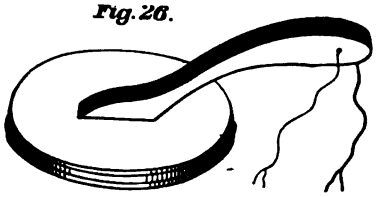
We found it advisable, however, to avoid placing metal over the sensitive area of the coils as had been done in the instrument shown in fig. 24.

In the balancing apparatus shown in fig. 25 (which is the most perfect one yet constructed), the lever to which the upper coil is attached is made of hard rubber.

In fig. 26 is shown the most convenient form of case yet devised for holding the exploring coils.

By invitation of Dr. Frank Hamilton, experiments were made at his office in New York, October 7, 1881, the instruments used being those shown in figs. 22, 23, 24.

Fig. 26.



As this was the first successful application of the Induction Balance to the discovery of the situation of a ball in the body, the position of which was previously unknown, I may be pardoned for entering somewhat into detail.

I shall quote from the *Medical Gazette*[28] of New York, an account of the experiments written by one of the witnesses:

[28] See *Medical Gazette*, Oct. 15, 1881, pp. 347-349.

"*The First Successful Application.*

On Friday, Oct. 7, by invitation, several medical gentlemen,[29] including the writer, met Prof. Bell at the house of Dr. Frank H. Hamilton, in this city, for the purpose of witnessing the practical application of his improved instrument.

The first person subjected to experiment was General Calvin E. Pratt, judge of the supreme court of the State of New York, and who is now a resident of Brooklyn. General Pratt, at the battle of Gaines' Mills, June, 1862, this being the second day of the famous seven days' retreat across the peninsula, received a ball in his left cheek, which penetrated through the nares and was lodged in the right antrum. Its presence at this time was recognized by his surgical attendant, Dr. Damainville, and its exact position has been known from that day until this, it having given rise at times to much pain and suffering.

General Pratt has been seen by Dr. Hamilton and Dr. Damainville occasionally from that time forward, and they have from time to time urged upon him the necessity of its removal. General Pratt, however, was anxious to know whether Professor Bell's instrument would indicate its presence at the same point as declared by his surgeons.

The results of the experiment were conclusive and entirely satisfactory to General Pratt, the response being heard distinctly, but rather feebly, by every person present in the room. The feebleness of the response was supposed to be due to the fact that, owing to its situation and the peculiar form of the instrument containing the induction coils, it was impossible to bring the centre of its surface very near the site of the ball, the ball being situated very near the depression at the ala of the nose.

The next patient was Col. B. F. Clayton, who received a wound at the battle of Cedar Mountain, Virginia, Aug. 9, 1862.

The missile was supposed to be an Enfield rifle ball, and the wound was supposed to be mortal by the medical director of General Banks' staff and his assistants. The ball passed through the

[29] "The following are the names of the medical gentlemen who were present, each one of whom verified personally the results and declared his entire satisfaction with every experiment that was made: J. C. Hutchinson, J. G. Johnson, and J. G. Allen, of Brooklyn; Elias Marsh, of Patterson, N. J.; Nathan Bozeman, J. H. Hunter, G. Durant, F. Delafield, L. Damainville, W. M. Chamberlain, J. H. Girdner, Frank H. Hamilton, and E. J. Bermingham, of New York."

sternal end of the left clavicle, and was supposed to have lodged in the muscles under the superior angle of the corresponding scapula. The injury was followed by complete paralysis of the left arm, continuing for a period of six months; and his arm has never yet been completely restored to its normal condition. He suffers a great portion of his time from pains in the arm, shoulder, and portions of the back.

Several small fragments of bone escaped through a fistulous orifice formed near the seat of the original wound.

About eighteen months later an abscess opened on the front of the chest below the fifth rib and to the left of the sternum. Through this sinus his surgeon was able to carry a probe upwards and backwards towards the top of the shoulder several inches, and which sinus was supposed then to communicate with the seat of the ball on the back.

Pleural adhesions were recognized by the medical attendants as having occurred in the upper part of the left thoracic cavity. He has been troubled occasionally ever since the injury with cough, expectoration, and violent palpitations of the heart. A suspicion has even been entertained that the fistulous canal which remained open a period of eighteen months, and then became permanently closed, communicated with the bronchial tubes, but at no time was a suspicion entertained by him or his medical attendants that the ball was not lodged in the back and there closely encysted.

We are disposed to mention as an evidence of Col. Clayton's loyalty and faithfulness as a soldier that within six months of the receipt of the injury, and while the wound was still discharging pus and blood, he returned to active duty with his regiment and remained in the field until the close of the war.

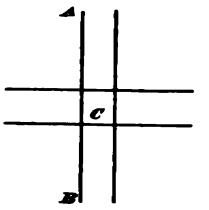
In the presence of the gentlemen assembled Col. Clayton exposed his chest, and Professor Bell proceeded to move the coils across that portion of his back where the ball was supposed to be situated, the colonel indicating the point underneath the superior angle of the scapula as that which had been fixed upon by himself and all the surgeons who had examined him as its exact seat. Although being buried underneath the scapula, they had not been able to verify their diagnosis by the sense of touch. Repeated examinations were made over this region, without any response, both by Professor Bell and several of the gentlemen who were present.

The instrument was then moved in every direction across the back and shoulders with the same result. There was an evident feeling of disappointment on the part of Professor Bell and all the gentlemen present, for no one entertained a doubt up to this moment that the situation of the ball was known and correctly stated by Col. Clayton.

It was not until the lapse of half an hour, and a thorough examination on the part of Professor Bell to determine if there was not some imperfection in the working of the apparatus, that it was suggested to move the instrument along the front of the chest.

This was done by Professor Bell, and immediately he exclaimed: 'I have found it!' And such was evidently the fact, as was verified by the personal examination through the telephone by every gentleman present. The response when the instrument was moved over the seat of the ball was loud and distinct, and left no room for doubt."

Fig. 27.



After all the visitors present had had the opportunity of verifying my discovery of the sonorous spot on the chest of Col. Clayton, experiments were made to determine as accurately as possible the exact position of the ball.

The exploring instrument (fig. 23) was first held over that part of the chest where the maximum sound was obtained. The instrument was then moved slowly towards the left until the sound could no longer be perceived. The position of the centre of the instrument was noted, and a vertical line (A B, fig. 27) was drawn with ink upon the skin through that point. This line indicated the boundary of the sonorous area towards the left. The experiment was then repeated by moving the instrument from the point of maximum sound towards the right, and also upwards and downwards, giving us the four boundary-lines shown in the diagram. (fig. 27). The bullet was thus located within a square, C, of about one inch.

"The exact position of the ball," as described in the *Medical Gazette*, "was found to be within the thorax, probably in immediate contact with the inner surface of the ribs, the point being a little to the left of the sternum, between the third and fourth ribs.

... inches above the cicatrix on the front of the ... long since closed, had evacuated itself, and ... cicatrix towards the left shoulder, which ... track of the original sinus."

...ments with Needles.

... from Washington and from all conveniences ... with Induction Balance apparatus, I ... verifying the indications of the Induction ... finding the exact depth at which a bullet lies ... This method was communicated through Dr. Woodward to the surgeons in attendance on President Garfield, and it was made the subject of a special paper presented to the French Academy of Sciences, Nov. 7, 1881.

This method, although involving extremely slight pain, would ordinarily be used only as a preliminary to an operation for the extraction ... arrangement is shown in fig. 28. A fine ... to one terminal of a telephone, and the ... contact with a plate B, preferably of the ... composing the needle. Place this metallic ... surface of the patient's skin and thrust the ... of the body where the bullet is believed ... the point of the needle makes contact with ... bullet C, a galvanic battery will be formed ... body, the two poles of which are respectively ... and the metallic plate B. Under these circum... be heard from the telephone each time the ... the needle. This has been verified by experi... buried in a joint of meat. The click, though ...able.

I have no doubt that this method of exploration alone, without the induction balance, would prove of great service upon a field of battle, where the employment of complicated apparatus is impossible. Mr. Thomas Gleason has recently communicated to me the particulars of an experiment he witnessed, in the course of which this method was tried upon a living subject. The surgeon who conducted the experiment was unable to obtain any response from the induction balance employed, although from certain indications apparent to the sense of touch he believed that the bullet was located in the part of the body submitted to experiment.

To verify his supposition a needle connected as above (fig. 28) was thrust into contact with the hard substance perceived, but no response was made by the telephone. The surgeon, however, believing that the bullet had been found, etherized his patient and proceeded with an operation, but discovered, when too late, that the bullet was not there.

Further Modifications of Induction Balance.

I sailed for Europe early in October, 1881, and have had no opportunity since of continuing my researches until quite recently. While I was in Europe, however, Mr. Sumner Tainter devised a new kind of induction balance which deserves mention here. The results obtained with this apparatus in its present form (fig. 29) are not to be compared with those produced by the best instruments described above, but there are undoubtedly great possibilities of future development.

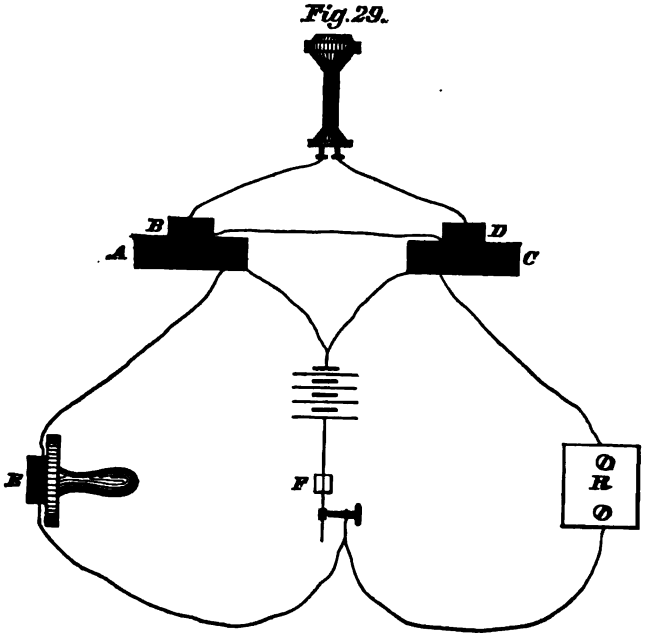
The important feature is that the exploring instrument E consists of *a single coil*, so that there is no possibility of any part of the explorer getting out of adjustment. All the adjustments are made upon the stationary part of the apparatus.

The current of the battery is divided between two equal circuits. One of the primary circuits contains the coil A and the exploring coil E, and the other circuit the coil C and a rheostat R. Coils A and C are exactly similar; and if the resistance introduced at R is equal to the resistance of the exploring coil E, an acoustic balance can be obtained by the adjustment of the secondary coils B D upon the primaries A C; but if the resistance introduced at R is different from that at E, Mr. Tainter states that no balance is possible.

When the apparatus is adjusted to silence the approach of a bullet to the coil E destroys the balance.

Although the great object of the researches that have been brought before you to-day has been to find that arrangement of balance which will detect a bullet at the greatest distance from the coils of the explorer, it must not be forgotten that in every case the instrument is more sensitive to the presence of a bullet placed *inside* the exploring coils than to one exterior to them. When, therefore, we seek the location of a bullet in one of the limbs, it

Fig. 29.



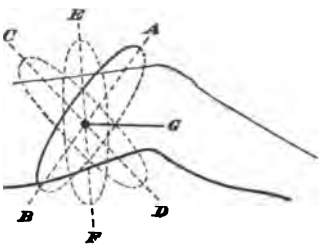
may be advisable to use an annular coil large enough to slip easily over the leg or arm, as the case may be.

In Mr. Tainter's arrangement the exploring coil E (fig. 29) might simply be a large ring consisting of a number of convolutions of thick wire which could be slipped over the limb, or the ring might consist of two coils, forming one side of a Hughes' induction balance.

In either case the loudest sound will be produced when the bullet is in the plane of the ring, and its exact location should be deduced from three observations. Suppose, for instance, that with the ring inclined in a particular direction the maximum sound is observed when the ring occupies the position A B (fig. 30). We know then

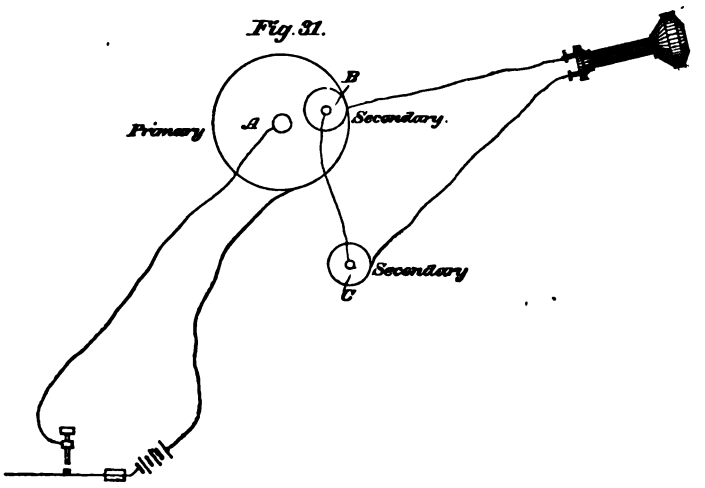
that the bullet is in that plane. Now, incline the ring in some other direction and explore again. Let the position of maximum sound be now C D. We know then that the bullet is somewhere on the straight line formed by the intersection of the planes A B and C D. It is only necessary then to make a third observation with the apparatus so inclined that the plane of the ring cuts this straight line, for instance, the position E F. The point of intersection of the three planes G is then the exact point occupied by the bullet.

Fig. 30.



I shall conclude this paper by the description of an experiment made in Newport, R. I., a few days ago. The results are so unprecedented in my experience that I feel they cannot be received as implicitly reliable until the experiments have been repeated and verified.

Fig. 31.



I had arranged upon a table three coils (as shown in fig. 31). The large flat primary coil A was connected with a battery of four Bunsen elements and an interrupter, as shown, and the two small secondaries of fine wire, B C, were connected with a telephone.

The secondary B was moved about on the primary A until a position of silence was obtained. Upon bringing a leaden bullet near C the balance was disturbed and a distinct sound produced from the telephone. There is nothing very strange about this when we know that the distance between A and C was only fifteen centimetres, so that C was well within the field of induction of A; but what did seem extraordinary was that the approach of the large steel blade of a penknife to the coil C produced no effect! The iron diaphragm of a hand telephone brought close up to the coil C produced no sensible disturbance of the balance, whereas a small disk of lead produced quite a marked effect. A disk of copper the size of a telephone diaphragm also produced a good effect, but the sound was not sensibly louder than that due to the small leaden disk. A diaphragm of zinc occasioned a feeble, but distinct, disturbance of the balance. It is unfortunately the case that in all the forms of induction balance described above, lead gives the poorest effect of all metals. If people would only make their bullets of silver or iron there would be no difficulty in finding them in any part of the body! In the apparatus shown in fig. 31, however, it seems (unless subsequent experiments should reveal some fallacy) that we have an arrangement which is sensitive to lead and not to iron, or, at all events, which is more markedly influenced by lead than iron.

It is hardly necessary to state that when the coil C was removed to a considerable distance from the primary A, no effect was produced by the approach of metal to the coil C.

I have in this paper brought before you an outline of a labor of love pursued through many anxious days and sleepless nights. However imperfect or disappointing may be the results so far achieved, they are sufficiently encouraging to enable us to look forward with confidence to the attainment of still greater perfection.

I hope to continue these researches in the future; and certainly no man can have a higher incentive to renewed exertion than the hope of relieving suffering and saving life.

APPENDIX.

NOTE 1. — Another mode of painless exploration suggested itself to my mind at this time, based upon the fact that a leaden bullet is much more opaque to light than the substances composing the human body.

I was aware of the fact that the ingenious M. Trouvée, of Paris, had, by means of his polyscope, produced a light inside a living fish, and was thus enabled in the dark to see the anatomical structure of the fish as it swam about in a vessel of water.

M. Trouvée had himself shown me a modified form of this apparatus for the illumination of the interior of the human stomach, and I understood him to say that when the instrument was used the body of the patient could be seen in the dark, faintly illuminated, like a Chinese lantern, and that the extent and location of tumors in the stomach could be detected on account of their great opacity.

It occurred to me that leaden bullets were certainly more opaque than tumors, and that a painless method of exploration might be based upon the observations of M. Trouvée.

It would evidently be impracticable, in the great majority of cases, to introduce into the stomach any illuminating apparatus; but if the light of a properly protected incandescent platinum spiral, introduced into the stomach, could produce effects visible from the outside — that is, if this feeble light could penetrate through the substance of one-half of the body — then why should not the intense light of an electric arc, or of a lime light, penetrate the whole body from one side to the other so as to produce similar effects?

The most feasible plan that occurred to me was to place an electric lamp at one end of a long opaque tube, and to apply the other end closely against the skin of the patient. The interior of the tube between two plate-glass diaphragms could be filled with a saturated solution of alum, or some other highly-absorbent transparent substance, so as to obstruct the passage of heat rays.

Of course, the whole apparatus was to be so arranged that no light from the lamp could escape into the room to interfere with the experiment.

Under these circumstances, the body of the patient, or at least a portion of it, should, in a dark room, appear self-luminous; and it seemed possible that the shadow of an embedded bullet might be projected upon the skin. The track of the bullet might also, perhaps, be discernible as an illuminated streak either more or less bright than the surrounding surface.

These considerations led me to try a number of experiments, which proved that the method was feasible where the bullet was very near the surface of the body. On account of the great and irregular refraction of light in passing through the tissues of the body, I doubt whether the shadow of a deeply-seated small bullet could be distinguishable upon the skin, unless, indeed, a very brilliant light emanating from a single point could be safely produced inside the body.

The following experiments bearing upon the subject were made in Boston, July 6, 1881, with the assistance of Mr. Wm. Schuyler Johnson:

Experiment 1.— Mr. Johnson placed in his mouth a glass cylinder containing a spiral of platinum wire, which was rendered incandescent by the passage of an electrical current. He also held in his cheek a small leaden bullet.

In a dark room the effect was very striking. The cheek appeared semi-transparent, and the location of the bullet was detected at a glance by a shadowy spot upon the skin.

Experiment 2.— A metallic glove button (not more than 4 or 5 mm. in diameter) was attached to the glass cylinder by a few turns of black thread passed round the cylinder.

In this case the effect, although not so marked as in the former experiment, was quite discernible, and a faint shadowy streak could be seen crossing the cheek, caused by the shadow of the black thread tied round the cylinder.

NOTE 2.— *Letter from Mr. George M. Hopkins, published in the New York Tribune, July* 11, 1881.

LETTERS FROM THE PEOPLE — THE BULLET FIRED BY GUITEAU.

A suggestion that the Induction Balance be used to discover its position in the President's body.

To the Editor of the Tribune:

SIR: The attempt upon President Garfield's life and the present condition of the sufferer have called forth the nation's deepest sympathies, and elicited very many suggestions as to the methods of promoting his comfort and assuring his recovery.

As one crisis after another has passed hope has risen, and the people wait with earnest desire for the last danger to pass away.

The attending and consulting surgeons state that it is impossible to predict the final result of the shot without locating the ball.

With all deference to the several methods proposed, I desire to suggest a method of ascertaining the position of the missile which will be painless and harmless, and which, in my experiments under conditions analogous to those under which it might be practised in the present instance, has proved successful. I refer to the use of the Induction Balance — a most delicate electrical instrument for detecting the presence of metals — a modified form of which could be easily applied in this case with a reasonable expectation of success. This instrument consists of two short glass cylinders, around each of which are wound two parallel coils of fine insulated copper wire.

One coil of each pair is included in a battery circuit, in which there is a clock microphone. The other pair is placed in a closed circuit with a receiving telephone. The two glass cylinders, with their encircling coils, may be widely separated.

The induction set up in the secondary or telephone circuit is balanced

by the reversal of one of the secondary coils, and so adjusted that the induction in one of the secondary coils exactly balances or neutralizes the induction in the other, so that when the ear is applied to the receiving telephone no sound is heard.

Now, by placing ever so small a piece of metal in one of the glass cylinders, the electrical balance is disturbed and the clock on the microphone is heard to tick loudly, thus indicating the presence of metal, and the same is true if the coil be placed in the vicinity of a piece of metal.

It occurred to me to try the effect of a lead bullet upon the instrument, placing it at different distances, and separating it from the coil by insulating material. The result exceeded my anticipations, as with a set of coils that were by no means sensitive I was able to locate the bullet with the coils raised a vertical distance of nearly two inches. With more sensitive apparatus it is more than probable that the bullet might be located, even though distant several inches, by passing a pair of coils over the President's back and abdomen; and by comparative tests the depth of the bullet might be ascertained.

GEORGE M. HOPKINS.

BROOKLYN, *July* 10, 1881.

NOTE 3.— *Letter from Mr. G. M. Hopkins to Private Secretary Brown, enclosing printed description of Hughes' Induction Balance.*

OFFICE OF THE SCIENTIFIC AMERICAN, No. 37 PARK ROW,
NEW YORK, *July* 11, 1881.

Mr. J. STANLEY BROWN,

Executive Mansion, Washington, D. C.:

MY DEAR SIR: I send herewith a full description of the Induction Balance as promised in my note of yesterday. I will send the apparatus for trial if desired.

I am certain that the bullet can be located with it if it is not too deeply seated.

Please advise Dr. Bliss.

Very respectfully,

GEO. M. HOPKINS,
of the Scientific American.

NOTE 4.— *Letter from Mr. Geo. M. Hopkins to Private Secretary Brown accompanying the Hughes' Induction Balance apparatus he forwarded to the Executive Mansion for trial.*

60 IRVING PLACE, *July* 13, 1881.

Mr. J. STANLEY BROWN:

DEAR SIR: I give below a few suggestions in regard to the use of the Induction Balance sent herewith.

Very respectfully,

GEO. M. HOPKINS.

Suggestions: Connect A to A, B to B, etc., as marked on the wires. Strength of current used, 15 or 20 volts.

A strong current extends the influence. Use the clock interrupter, and place a drop of mercury in the vulcanite cup to make connection.

The coils are now adjusted to "silence," but will probably have to be readjusted on their arrival in Washington; as a $\frac{1}{100}$ inch movement of one of the coils will throw it out of adjustment, it will be seen that the adjustment is a matter of great nicety. Two telephone receivers should be employed, and the most acute sense of hearing is required to distinguish the ticking when the ball is two inches distant from the coils.

The shorter instrument is intended for application to the patient should it be considered sufficiently sensitive to warrant the experiment.

As a preliminary experiment pass the induction balance over the pocket containing coins, keys, etc.; also over buttons, buckles, etc., attached to the garments.

Lead is the poorest of all metals to locate with this instrument. If the ball were of iron it could be readily found at a distance of 3 or 4 inches.

NOTE 5.— *Telegram from Professor Newcomb.*

EXECUTIVE MANSION,
WASHINGTON, D. C., *July* 13, 1881.

To Prof. A. G. BELL,

Care of Chas. Williams, 109 Court St., Boston, Mass.:

Perhaps small core for coil no larger than bullet, with very fine wire, might give best effect. Shall telegraph to Rowland to know best theoretical form. Telegraph me or Brown, care Executive Mansion, what train you take.

S. NEWCOMB.

NOTE 6.— *Telegram from Prof. John Trowbridge.*

BAR HARBOR, MAINE, *July* 16, 1881.

To Secretary Brown,

White House, Washington, D. C., for Professor Bell:

Make resistance of secondary coils equal to telephone. Put large number of turns of wire on primary and secondary coils. Primary coils long compared with width. Put secondary coils around middle of primary coils.

JOHN TROWBRIDGE.

NOTE 7.— *Telegram and letter from Professor Rowland to Professor Newcomb.*

HUNTER, N. Y., *July* 14, 1881.

To Prof. SIMON NEWCOMB,

Executive Mansion, Washington, D. C.:

Telegram just received. Make cores four inches long, with six layers of number twenty wire; diameter an inch and one-half. Make outer coil half an inch wide and half an inch thick, of finest wire, and slip over inner coil at its end.

H. A. ROWLAND.

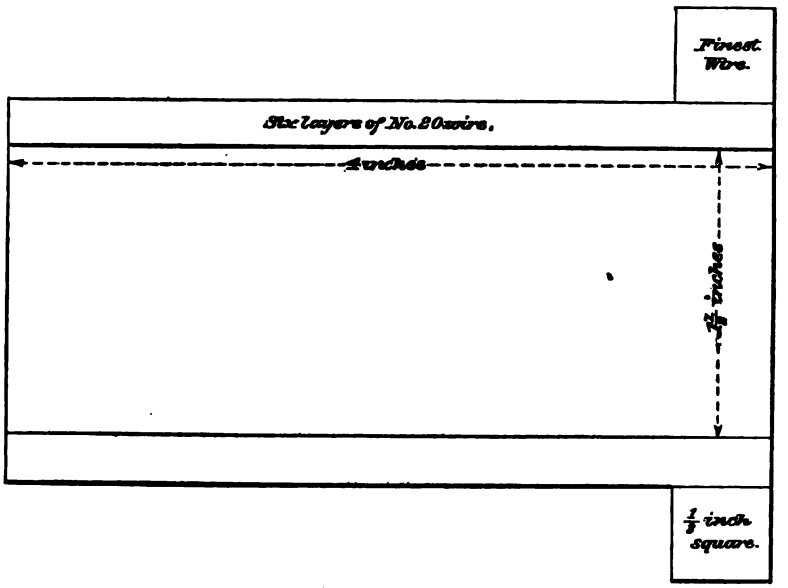
Letter following above telegram.

HUNTER, *July* 14, '81.

Professor NEWCOMB:

DEAR SIR: When your telegram arrived I had gone to the Hotel Kaaterskill, about ten miles from here, to stay over night. The telegram was sent back to the office to be sent to me, and my friends thought I had received it; but the stupid man here simply kept it and would not send it, because he had no authority to send it farther.

I have telegraphed back a reply. The dimensions should probably be about as follows:



Of course, the two must be precisely alike, and the induction coëfficients of one should be made to vary by a screw, which would move one of the secondary coils nearer to or farther from the end of the primary.

Yours truly,

HENRY A. ROWLAND.

P. S.—For these dimensions a telephone with many turns of fine wire in its coil would be best.

NOTE 8.— *Communications with Prof. D. E. Hughes, of London, England.*

(*a.*) *Cablegram to Mr. Preece, Superintendent Postal Telegraph, London, England.*

WASHINGTON, *July* 15, 1881.

To PREECE,
London:

Can Hughes suggest form of Induction Balance to locate leaden bullet in President? If so, cable at my expense.

GRAHAM BELL.

(*b.*) *Telegraphic reply to above.*

WHITNEY,
Washington, D. C.:

Preece says to Graham Bell, Washington: "Interesting and hopeful letter from Hughes with diagrams sent you to-day."

W. GREEN,
Pres. West. Union Telegraph Co.

(*c.*) *Note from Mr. Preece enclosing letter from Professor Hughes.*

GENERAL POST OFFICE, LONDON, ENGLAND, 19 *July*, 1881.

MY DEAR BELL: The enclosed very interesting letter from Hughes, will enable you to make some experiments, which I trust will result in success.

Yours, sincerely,

W. H. PREECE.

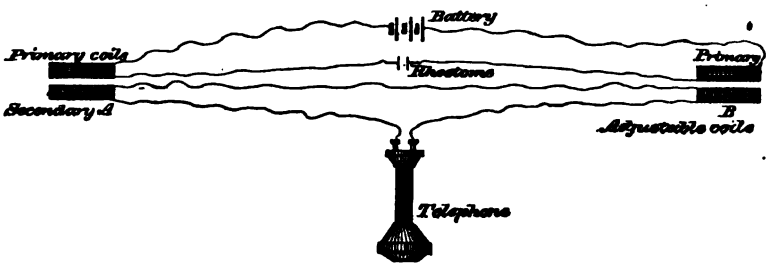
Prof. ALEXANDER GRAHAM BELL,
Washington, D. C.

(*d.*) *Professor Hughes to Mr. Preece.*

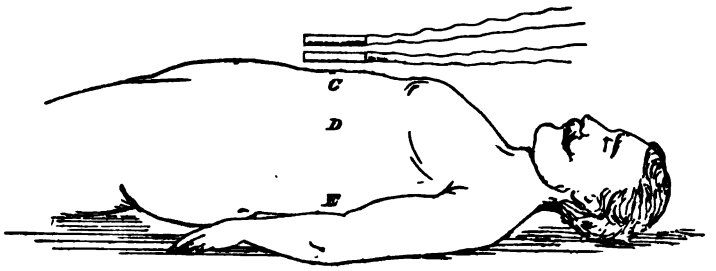
108 GT. PORTLAND STREET, W. LONDON, *July* 18, 1881.

DEAR PREECE: In reply to yours of to-day enclosing telegram from Prof. Graham Bell, I believe a specially-constructed Induction Balance could be made to locate the leaden bullet in President Garfield, provided the ball was nearer to one side of his body than the other.

Suppose we have two flat superposed coils on a single reel, so that these two coils form a single one as regards their relative distance; these coils, if connected with the usual adjusting coils of my balance, could be reduced to silence; then, on moving these coils near a metallic body, sounds would be heard. Thus the only difference would be instead of as usual taking the metallic body to the coils upon a fixed table, we take the coils to a fixed body in which we suppose metal, such as a bullet, to be hid.



Now, suppose we take coil A and move it over a body with a bullet, thus:



If bullet near C, we should hear it when coils just above it; at E, when the coils were below; but if bullet in the centre of the body, then we should have equal or probably no indications.

All this could be most easily done and with some results if the bullet had been of copper or silver, but lead has such a high resistance and gives, consequently, very feeble tones.

We can easily hear a copper penny at some inches distant from the coils, but lead requires that it should not be more than one or two inches distant: consequently, more sensitive coils would be required or a larger battery.

I think the experiment a hopeful one, and above all that the coils are easily made, and simple experiments could be first made to find a bullet in a mass of cotton, etc.

The microphone is invaluable as a probe for bullets. I made the first for Sir Henry Thomson to find out stone in bladder. But it is even more applicable to bullets. It consists of a simple hammer and anvil microphone

adjusted upon the handle of the ordinary probe. The instant this probe touches any hard substance a loud sharp click is heard in the telephone. The smallest shot can thus be heard, and there is a very distinctive tone between when the probe strikes a bone or bullet. This instrument is well known to surgeons, so I need not say more about it.

If you write to Professor Bell, please enclose this, as I should be pleased to hear of his success.

Sincerely yours,

D. E. HUGHES.

W. H. PREECE, F. R. S.

NOTE 9.— *Letter from Mr. Charles E. Buell to Col. Rockwell.*

Col. ROCKWELL:

DEAR SIR: Please suggest for me that in an electrical test to locate the bullet it will only be done with certainty by obtaining a balance, which can be done as follows: When the loudest tick is obtained by placing the inducing coil in the vicinity of the embedded bullet, then move another bullet of equal size towards the other coil of the bridge until a balance is had and no tick is audible, when the embedded bullet will be known to be a like distance from its respective coil.

CHARLES E. BUELL,
Electrical Engineer, New Haven, Conn.

NOTE 10.— *Note from Dr. Chichester A. Bell.*

N. Y. CENTRAL R. R., BETWEEN SYRACUSE AND BUFFALO,
July 16, 1881.

DEAR A. G. B.: If your coils are symmetrically arranged, should it not be possible to balance the effect of a bullet on one side by a bullet placed in a corresponding position on the other?

Probably you have thought of this before, but, if not, it may be worth a three-cent stamp.

Yours, truly,

CHICHESTER A. BELL.

NOTE 11.— *Note and telegram from Mr. J. H. C. Watts.*

BALTO., *July* 21, 1881.

Prof. A. GRAHAM BELL,
1221 *Conn. Ave., Washington, D. C.:*

Am working hard at our problem. Succeeded in reaching full three inches last night, but owing, I think, to my battery weakening somewhat, can hardly reach so far now. Won't you please advise me how you are getting along, and oblige,

Yours, very truly,

J. H. C. WATTS.

Telegram.

BALTIMORE, *July* 23, 1881.

Prof. A. G. BELL,
Washington, D. C.:

Have been unable to obtain satisfactory results since first trial and probably yours is at least as good as mine.

J. H. C. WATTS.

NOTE 12.— *Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 23, 1881.

Dr. BLISS,
Executive Mansion:

MY DEAR SIR: You were kind enough to suggest that preliminary experiments should be made with the Induction Balance upon the person of Lieutenant Simpson, whose address you gave me. I accordingly communicated with the lieutenant, and he came to my laboratory last night in company with Dr. Stanton.

The new instruments we had hoped to use were not complete, so we had to content ourselves with the apparatus you tested the other day.

Upon passing the coils over the back of the lieutenant, it was found that at one spot a feeble sound made its appearance — too feeble, however, to be entirely satisfactory as evidence of the presence of a ball. I find that very feeble sounds like that heard are easily conjured up by imagination and expectancy, but the following facts seem to indicate that in this case the sound was due to an external cause — probably to the presence of a very deeply embedded bullet:

1. The sound, although it could not always be distinguished, uniformly appeared, when audible, *at the same spot.*

2. It was heard independently by Mr. Tainter, Dr. Stanton, and myself at the same spot. (If I recollect rightly, the lieutenant himself also located the sound, but my father could hear nothing.)

3. A blindfold test was then made. Mr. Tainter closed his eyes and turned away while the coils were moved over different parts of the lieutenant's back. The moment the sonorous spot was covered Mr. Tainter declared he could hear the sound. We are to repeat the experiments with our new instruments as soon as they are completed, and I shall report progress.

Yours, truly,

ALEXANDER GRAHAM BELL.

NOTE 13.—*Letter to Dr. Bliss.*

WASHINGTON, *July* 27, 1881.

Dr. BLISS:

MY DEAR SIR: Permit me to make a few remarks upon the value of the indications of the Induction Balance in determining the location of a lead bullet embedded in the human body.

Positive deductions concerning the location of the ball can only be safely made from positive indications of its presence. Negative indications may mean anything.

For instance: If we hear a sound from the telephone connected with the balance every time the exploring coil is passed over a certain part of the body, this may be taken as proof positive that a metallic mass is to be found in the neighborhood of the sonorous spot. If, on the other hand, we fail to obtain audible effects, we are not entitled to conclude that the bullet is absent from the part explored, or that it is embedded at a greater distance below the surface than the penetrating distance of the instrument used, for the bullet may be within any part of the area explored and close to the surface without affecting the balance if it is flattened, and if it is so located that the plane of its face is nearly perpendicular to the plane of the face of the exploring coil.

A few words may not be out of place here concerning the experiment made last night upon the person of the President.

Before entering the President's room, I applied the telephone to my ear, and heard a peculiar spluttering sound which it was found impossible to extinguish by any adjustment of the coils. I satisfied myself, however, that the arrangement was in a sensitive condition, by holding near it a flattened bullet. The hearing distance appeared to be about four cm. When the exploring coil was passed over the back of the President near the spine, no definite pulsation of the sound was heard; but when the coil was first passed over a portion of the front part of the abdomen near the right side, I heard at one point a sharp and sudden reinforcement of the sound. Upon moving the coil backwards and forwards a number of times over the same spot, I was unable to verify the observation. I should state here that when a perfect balance is not obtained, it is not safe to place reliance upon a single observation of a pulsation in the sound, as a similar effect might be caused by an accidental irregularity in the vibration of the instrument used to interrupt the electrical current.

The sound heard was distinct and well marked, but it would not be safe to conclude that it was due to the presence of the bullet, unless the effect could be reproduced a number of times and always at the same spot.

I think I mentioned to you that it was discovered yesterday morning that the application of a tin foil "condenser" to the Induction Balance markedly influenced the hearing distance, increasing it in our experiments by about one centimetre.

A condenser was therefore connected to the arrangement used last night, but it has since been found that the condenser was only connected to one side of the balance, instead of to both. This mistake is enough to account for the difficulty experienced in adjusting the coils so as to obtain an acoustic balance, and for the observed reduction in the hearing distance.

If it is of importance to locate the bullet at once, I would recommend an immediate repetition of the experiment with the condenser properly arranged.

Whatever other results might be obtained, I feel sure we could, at all

events, settle immediately whether the sound I heard was due to the presence of the bullet or was an accidental phenomenon.

If it is not important to locate the bullet at the present time, it might be well to postpone a repetition of the experiment, so as to give Mr. Tainter and myself more time to improve our apparatus.

Yours, truly,
ALEXANDER GRAHAM BELL.

NOTE 14.—*Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 31, 1881.

MY DEAR DR. BLISS: I write to let you know that my new form of Induction Balance gives brilliant promise of success. The indications with a flattened bullet are well marked and distinct at a distance of three inches, and audible effects can even be distinguished at five inches, but beyond three inches, silence and the greatest attention are requisite. Effects are produced at about two inches, when the bullet is held with its edge towards the instrument—a position that gave no results with our former apparatus.

Altogether I feel very much encouraged. The apparatus in its present form is a very clumsy affair, the surface that would be applied to the person of the President measuring seven inches by four. I hope to reduce the size of the apparatus very greatly in a day or two. In the meantime, should any necessity arise for an experiment upon the President, we have much better chances of success than at any previous time.

Yours, very truly,
ALEXANDER GRAHAM BELL.

NOTE 15.—*Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 31, 1881.

Dr. BLISS,
Executive Mansion:

DEAR SIR: We have made experiments this evening upon the person of Private John McGill, an old soldier, who was wounded at the battle of Gaines' Mill, in 1862, and who still carries the ball that shot him.

I found no difficulty in finding a sonorous spot in his back, where undoubtedly the bullet lies embedded.

Mr. Tainter also located the bullet in the same place. Upon pressing with the fingers upon the spot a lump could be felt between two of the ribs. We experimented upon this same man yesterday, using a similar form of Induction Balance to that we tried upon the person of the President the other day, but could obtain no indications.

The new form of Induction Balance is so sensitive that a new difficulty is introduced by the effects produced by large metallic masses — for instance, gas lustres, iron fire-places, etc.

I think also that the earth's magnetism affects the result. We shall investigate these causes of disturbance to-night.

Yours, sincerely,
ALEXANDER GRAHAM BELL.

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16. *Report to the surgeons published in the daily papers Aug.* 2, 1881.

VOLTA LABORATORY, 1221 CONN. AVE.,
WASHINGTON, D. C., *Aug.* 1, 1881.

To the Surgeons in attendance upon President Garfield:

GENTLEMEN: I beg to submit for your information a brief statement of the results obtained with the new form of Induction Balance in the experiments made this morning for the purpose of locating the bullet in the person of the President. The instrument was tested for sensitiveness several times during the course of the experiments, and it was found to respond well to the presentation of a flattened bullet at a distance of about four inches from the coils.

When the exploring coils were passed over that part of the abdomen where a sonorous spot was observed in the experiments made on July 26, a feeble tone was perceived, but the effect was audible a considerable distance around this spot. The sounds were too feeble to be entirely satisfactory, as I had reason to expect from the extreme sensitiveness of the instrument a much more marked effect. In order to ascertain whether similar sounds might not be obtained in other localities I explored the whole right side and back below the point of entrance of the bullet, but no part gave indications of the presence of metal, except an area of about two inches in diameter, containing within it the spot previously found to be sonorous. The experiments were repeated by Mr. Tainter, who obtained exactly corresponding results. We are therefore justified in concluding that the ball is located within the above-named area.

In our preliminary experiments we found that a bullet like the one in question when in its normal shape produced no audible effect beyond a distance of two and a half inches, while the same bullet, flattened and presented with its face parallel to the plane of the coils, gave indications up to a distance of five inches. The same flattened bullet, held with its face perpendicular to the plane of the coils, produced no sound beyond a distance of one inch. The facts show that, in ignorance of the actual shape and mode of presentation of the bullet to the exploring instrument, the depth at which the bullet lies beneath the surface cannot be determined from our experiments.

I am, gentlemen, yours truly,
ALEXANDER GRAHAM BELL.

On Secondary Batteries. By George F. Barker, of Philadelphia, Pa.

[ABSTRACT.]

A SECONDARY battery is one which produces a current in consequence of a previous electrolytic condensation or polarization upon the surfaces of its plates. Electrolytic polarization and the production of a secondary current by means of it, are almost as old as the voltaic battery itself. In 1801, Gautherot, having used platinum or silver wires as electrodes in a solution of salt, noticed a momentary inverse current when the battery was replaced by a galvanometer. Ritter observed the same phenomenon in 1803 with gold wires. He constructed a secondary battery with pieces of gold separated by cloth disks moistened with salt water. After contact for some seconds with a Volta's pile of more pairs than itself, it produced a current to which Ritter gave the name of secondary current. Platinum, gold and silver, gave a more marked result than copper, brass, iron or bismuth; and no effect at all was obtained from lead, tin or zinc. Fifty disks of copper separated by disks of cloth moistened with salt or sal ammoniac solution, when charged with a Volta's pile of 100 pairs, gave a current which decomposed water and produced all the effects of the primary current. Volta himself, as well as Marianini, explained the results by supposing that the decomposition of the salt produced acidic and basic deposits upon the metal disks; a view which Becquerel apparently confirmed by immersing a platinum plate in an acid and another in an alkali and noting that the current was the same in direction as the secondary current. But in 1826 De la Rive obtained the secondary effect from platinum plates immersed in dilute sulphuric acid; and since no basic or acidic deposits were now possible, he regarded the phenomenon as a physical one due to a special or polarized condition of the plates themselves. The current he called a polarization current. In 1833–4, Faraday's researches on electro-chemical decomposition definitely established the proportionality of the current to the amount of electrolyte decomposed. He observed the "peculiar state" of the plates in his voltameter; and even noticed that when lead acetate was electrolyzed, metallic lead was deposited on the negative plate and lead peroxide on the positive; but he offered no explanation of these phenomena. In 1842, Grove, following

out an observation of Matteucci, constructed a gas battery in which the current was produced from two plates of platinum immersed in oxygen and hydrogen gases respectively. In 1852, C. W. Siemens used two carbon plates in lead acetate solution as a secondary battery; obtaining, by the action of the current, lead on one of these plates and lead peroxide on the other. One cell of this battery decomposed water; so that the electromotive force of the current must have been nearly two volts.

In 1859, Planté began an elaborate research upon this secondary action, studying carefully the influence of the metal and of the solution employed. The maximum effect was given by lead plates immersed in dilute sulphuric acid; and in 1860, he produced secondary batteries of remarkable power by rolling two sheets of lead together, a strip of insulating material being between them, and placing them in a ten per cent sulphuric acid. When connected with a battery, the oxygen evolved upon the positive plate attacked it and converted its surface into peroxide; the hydrogen for the most part escaping. When now the current was reversed, the hydrogen reduced this peroxide to finely divided metallic lead and the oxygen attacked the other plate, producing peroxide. By such alternations, the metal may finally be corroded to any depth and the amount of finely divided lead and peroxide indefinitely increased. In Planté's experiments this forming process required three months. In 1881, Faure brought out a secondary battery in which, as in Planté's, lead plates in sulphuric acid were employed. But in order to avoid the tedious process of "forming," in which the deposits upon the plates were produced by electric action, Faure covered the plates at the outset with a layer of minium or red lead. Upon passing the current, this red lead was reduced to metallic lead on the one plate and oxidized to peroxide on the other; thus producing the battery charged and ready for use in a single operation requiring but a few hours.

The theory of the storage of energy by secondary batteries is not difficult. Energy is capacity of doing work and work is force acting through distance. Whenever two attracting bodies are separated work is done upon the system and energy is absorbed by it; and this whether the attraction overcome is mechanical, molecular or atomic. When an electrolyte is decomposed by an electric current, the current yields energy in proportion to the amount of the ions separated. The quantity of lead peroxide

formed in a secondary battery represents the amount of energy stored from the current. So soon, however, as the separating force ceases to act, the ions reunite and the stored energy becomes again free. Since the amount of energy thus set free, like that originally stored up, is proportional to the quantity of matter concerned, it is clear that the amount of electricity obtained from a given secondary battery is proportional to the amount of electrolytic products deposited upon its plates. In theory, the electrical energy set free should equal that originally stored up; but in practice this result is never attained.

In its primary expression the chemistry of the secondary battery is simple. In the form made by Grove, two platinum plates are placed in acidulated water. Under the action of the current, oxygen is set free on one of these and hydrogen on the other,[1] and the plates are said to be polarized. The electromotive force between these plates is the sum of the contact-potentials involved. Its value is 1.45 volts and it measures the chemical attraction between oxygen and hydrogen. If now the two plates be connected by a wire, the oxygen and the hydrogen recombine and the energy of combination appears in the form of a secondary current. In general, however, the electrolytic action is accompanied by secondary chemical changes which render the phenomenon much more complicated. In the lead battery, for example, secondary products appear at both electrodes. The oxygen set free at the anode combines with the lead, the final product there being peroxide. The hydrogen evolved at the cathode reduces the oxide which it finds there and produces metallic lead. As before, the electromotive force in the battery is the sum of the contact-potentials; and this, in the case of lead and lead peroxide, is a little above two volts.[2] When now the plates are connected by a conductor, the entire process is reversed. Water is decomposed, and, since the immersed portion of the plates changes sign on discharge, the hydrogen now goes to the peroxide surface and oxygen to the finely divided metallic lead. The former is deoxidized to monoxide and the latter oxidized to the same condition. Simultaneously, a current flows through the

[1] Undoubtedly the oxygen is produced by a secondary reaction.

[2] Planté observes that, after complete charging, the electromotive force may rise above two volts at the instant of removing the battery. But as it falls to this value at once on closing the circuit and remains so, he attributes the first result to the temporary production of a peroxidized substance, as hydrogen peroxide or persulphuric acid, at the anode.

conductor, the electromotive force of which is two volts, the strength of which is the ratio of this electromotive force to the resistances in circuit, and the duration of which depends upon the amount of secondary products deposited upon the plates during the charging. Beside this, however, another action takes place in the secondary battery which is of great importance. The sulphuric acid in which the plates are immersed attacks both the peroxide on the one plate and the metallic lead on the other and converts both into lead sulphate. Moreover, as fast as the monoxide is produced by the discharge of the battery, it also is converted into sulphate. So that upon complete discharge, the plates are precisely alike, both being covered with lead sulphate.

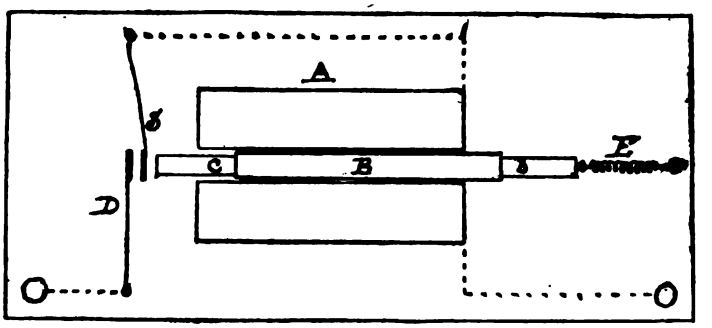
The secondary batteries employed for experiment were those of Planté and Faure. Of the former type, two cells of the larger size made by Breguet were used. Of the latter, one cell of medium size made by Reynier, and thirty-four cells of the size exhibited at the Electrical Exhibition at Paris, were experimented with. The battery of thirty-four cells was made in January under my direction.[3] Each cell consisted of six plates, each 20 by 60 centimetres, the lead being a millimetre thick. The exposed surface of each plate, therefore, was nearly 0.25 of a square metre, and of each cell about 1.50 square metres. For the whole battery the surface was about 50 square metres (exactly 48.96). Each plate was covered on both sides with a coating of red lead about a millimetre thick made into a paste with water. Parchment paper was then folded over the whole, and over this a covering of felt sewed together at the edges. The plates were then folded transversely and each pair intercalated. Heavy lead terminals were attached to each plate, and three pairs of plates were placed in each box, the terminals of one plate of each pair being united together and furnished with a connector. The boxes were lead-lined and measured 31 cm. in length, 25 in depth and 11 in width. One of the two sets of plates was in contact with the lead lining, the other was insulated. The latter was always made the positive end. The total weight of a single cell was 17 kilograms.[4]

[3] This battery, as well as the cut-out, Fig. 1, was made by Jas. W. Queen & Co., of Philadelphia.

[4] Mr. W. Lachlan, the electrical engineer of the "Light and Force Company," of New York, has kindly furnished me the following figures concerning the Faure batteries of improved construction recently imported in the steamer Labrador. Four types of Faure batteries are made in Paris, called A, B, C and D. Type A consists of seven positive plates, each 37 × 23.5 cm. and 3 mm. thick, perforated with holes 6 mm. in diameter 10 mm. apart, alternated with seven negative plates of the same size, but

The charging current was generated by a Gramme machine whose field was maintained by an auxiliary dynamo. The maximum obtainable electromotive force of this current was 85 volts. In general, however, a much less electromotive force was used, sufficient only to give a current of from 10 to 20 amperes through the thirty-four cells arranged in series. Currents as high as 45 amperes, however, were occasionally employed. At the outset, considerable difficulty was experienced from the discharge of the battery when from any accidental cause the current fell below the potential of the thirty-four cells. The wear of the brushes of the auxiliary dynamo sometimes opened its circuit and thus destroyed the magnetic field of the Gramme. There being now no counter elec-

FIG. 1.



tromotive force to oppose it, the secondary battery current flowed back through the machine, discharging the battery completely. A simple device was arranged to prevent loss of the charge in this way should the charging current become weakened. This device is represented in fig. 1, where A is a coil of No. 10 copper

only 0.5 mm. thick, and similarly perforated. The seven positive plates weigh 16.66 kilograms; the seven negative ones 2.77 kilograms. The red lead spread on their surfaces weighs 23.13 kilograms, the two exterior plates being covered only on their interior surfaces, so that there are twenty-six active surfaces in all. Each plate has an electrode 7 cm. wide. The box is 45 × 20 × 40 cm. in size, is pitched throughout, and weighs with its cover, 10 kilograms. The battery complete, ready for charging, weighs 63 kilograms. In type C there are five positive plates each 24 × 19 cm., 3 mm. thick, and four negative plates 0.5 mm. thick, both perforated with holes 5 mm. in diameter, and a centimetre apart. The positive plates weigh 7.14 kilos, the negative 0.888 kilos. Sixteen surfaces are covered with red lead, weighing 8.28 kilos. The electrodes are 3.5 cm. wide. The box weighs 3 kilos., and the battery complete 20.37 kilos. Each cell of type A contains, it is claimed, when fully charged, 375 ampere-hours of current delivered at an electromotive force of 2 volts. Hence it will furnish, for example, a current of 25 amperes for fifteen hours. Type C contains 150 ampere hours. Types B and D are round cells in jars of earthenware. The former weighs 35 kilos., and contains 225 ampere-hours; the latter 8 kilos. and contains 75.

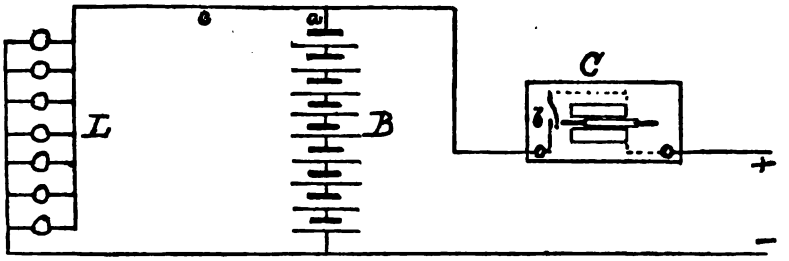
wire. Within its core is a rod of iron B, prolonged at both ends by brass rods of smaller diameter, *b* and *c*, these being supported upon rollers. Opposite the end of one of these rods is a flat spring S, faced with platinum and making contact when depressed with a similar brass strip D, also faced with platinum. The course of the current is shown by the dotted lines. When the circuit is closed between S and D, the current traversing the coil draws the iron bar strongly into its axis, and the end of the brass rod pressing against S holds the circuit closed. Should the current weaken, the spring S reacts against the rod pushing it back and opening the circuit. By means of an auxiliary spiral spring E, the instrument may be adjusted to any required current-strength. It would seem that by no possibility can the accumulated charge in the battery be lost, when this automatic cut-out is in circuit. As now described, the coil is of large wire, and the entire current passes through it. But a second form of it has been used in which the coil is made of finer wire and is placed in a shunt circuit as shown in fig. 3. In both cases of course, the entire current passes through the contact springs.[5]

After charging, the current from the secondary battery was used to maintain seventeen Edison incandescent lamps of the B pattern. More frequently, however, the battery was used in connection with the Gramme machine. The source of power being an Otto gas engine which, at best, has an explosion only every fourth stroke, the motion appears very irregular when tested by so delicate a method. As every explosion increases the speed, the higher electromotive force developed increases for an instant the brightness of the carbon filament, producing a marked pulsation in the light. If now a secondary battery of nearly the same electromotive force be placed in multiple circuit with the machine and lamps, it acts as an equalizer, and destroys entirely the pulsations. Figs. 2 and 3 show the arrangement actually employed, in which B is the secondary battery, and L the lamps; the automatic cut-out being at C. When the circuit is opened at *a*, *b* and *c* being closed, the lamps are fed by the machine alone; when opened at *b*, *a* and *c* being closed, by the battery alone; when at *c*, *a* and *b* being closed, the machine charges the battery. When all three are closed, the lamps are fed by the machine and bat-

[5] Sir Wm. Thomson has devised a polarized cut-out for this same purpose. He exhibited it at the York meeting of the British Association in September last.

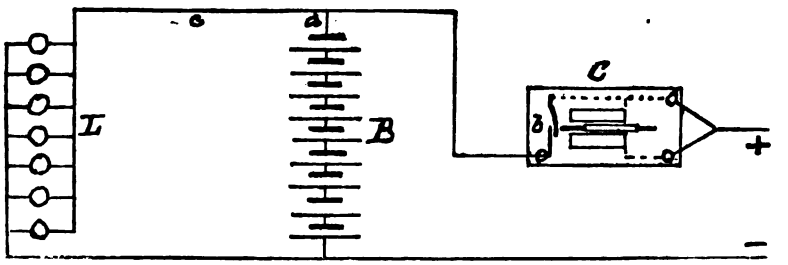
tery together, the latter acting as an equalizer. A galvanometer inserted at a, c being opened and b closed, indicates the charging current; when c is closed and b open, the discharge current; and when b and c are both closed, the equalizing current. If the electromotive force of the machine-current be but slightly in excess of the battery current, the deflection to the right in the

FIG. 2.



first case will about equal that to the left in the second; so that in the third case the deflection is practically zero, not the least spark appearing on opening at a; and this, though the pulsations are completely destroyed, the light being perfectly steady. The

FIG. 3.



advantage of a much more agreeable light is thus secured; but in addition the life of the lamps must be increased by the use of this steadier current, as it has been shown to be by using the secondary battery alone.

On the 27th of February the battery was charged continuously for fourteen hours, or until hydrogen was freely evolved from each of the thirty-two cells employed. It then gave an electromotive force of 59.3 Daniell cells; or calling the Daniell 1.08 volts, of

64.04 volts. On the tenth of June, the same thirty-two cells gave an electromotive force of 56.7 Daniell's, or 61.2 volts. On the 24th of June, their electromotive force was 54.3 Daniells', or 58.6 volts. It may be assumed then that the average electromotive force of a single cell was not far from two volts. This value has never been exceeded in any of the measurements made, either on single cells or on groups of cells. When first made, the resistance of the single cells was low, about 0.02 ohm; but subsequently the internal resistance increased considerably, rising even to twice this value.

The chief defect of the secondary lead battery is the local action which takes place in it. By local action is meant any deterioration in the efficiency of a battery which occurs when it is left on open circuit. This defect was noticed by Planté, who says: "If the lead peroxide deposited on the positive plate had no tendency to spontaneous reduction in the acidulated water, by forming a local circuit with the metal that it covers, the preservation of the charge taken by the secondary couple should be indefinite. But this peroxide is reduced, and with the more facility as the layer of it is thinner. In a new secondary couple the charge cannot be preserved." In a well formed battery he attributes the preservation of the charge to a reduction of the superficial layer to monoxide, which acts as a protecting coating. Another local action, however, takes place in this battery which appears to be of more importance. This is the action of the sulphuric acid upon the products of electrolysis. The finely divided lead on the negative plate, as well as the peroxide upon the positive, is alike converted into sulphate by this action. When the circuit is closed this action is normal; but it takes place equally well when the circuit is open. It is then abnormal and of course injurious. The formation of lead sulphate in the secondary battery has been studied by Gladstone and Tribe.[6] They found that with a Planté cell the whole of the peroxide formed on charging was converted into white sulphate within seventeen hours. The same fact was observed with the Faure battery. So far however, from regarding this formation of sulphate as wholly injurious, they regard it as "absolutely requisite in order that the charge should be retained for a sufficient time to be practically available." They believe that the insoluble lead sulphate clogs up the interstices of the

[6] Nature, XXV, 221, 461; XXVI, 251, 1882.

peroxide and after a while forms an almost impermeable (*sic*) coating between it and the lead plate; so that the mass of peroxide which is in contact with the lead plate expends its energy slowly, while the same mass of peroxide, brought into connection through its own lead plate with another lead plate at a distance, expends its energy even through the sulphuric acid, in a tenth or a hundredth part of the time.

My experiments with the Faure battery above described confirm entirely those of Gladstone and Tribe as to the formation of lead sulphate. In several cases, the acid in charged cells disappeared entirely on standing so that the water was tasteless; and in some instances this was repeated three times with the same set of plates. This action increased very greatly the internal resistance, first by replacing the acid by water and second by forming the higher resistant sulphate. On examining the plates, lead sulphate formed the entire coating upon both of them. On attempting now to recharge such a cell, this notable increase in the resistance showed itself in the reduced current transmitted by the same electromotive force. Gradually, however, the resistance fell and the current finally regained its normal strength. In one instance, the charging current rose from 2 amperes to 12, in four hours, 28 cells being in circuit, arranged in series. It was observed, however, that when the current was first applied, torrents of gas were evolved from both plates; and that as the charging process went on the oxygen was first completely absorbed and then the hydrogen. When the charge was nearly complete the evolution of hydrogen reappeared. There would seem, therefore, to be a very considerable loss of energy, due to the resistance consequent upon the formation of sulphate. In the first place the current energy is wasted as heat in the cells; and in the second, a large portion of it is dissipated in the evolved gases.

Another defect of considerable magnitude is developed when a number of secondary cells is placed in series, and is due to a want of uniformity in their capacity for storage. This arises probably first, from the difficulty of constructing them exactly alike originally, and second, from the impossibility of uniform action among them on charge and discharge. The difference in the cells of the same battery even, when they all have been treated precisely alike, is surprising. On the 14th of June, thirty-two cells were freshly charged, and the current strength from each cell measured on a tangent galvanometer. Eleven cells gave 80° deflection each,

seventeen gave 79° each, one gave 78°, one 77°, one 75°, and one 20°. When seventeen Edison B lamps were placed in circuit, the discharge deflection of the thirty-two cells in series was 67°. On the following day the battery was charged again, and, after an hour's use on the lamp circuit, was again tested. Those giving a deflection of 87° were two; of 86°, two; of 84°, two; of 83°, one; of 81°, one; of 78°, five; of 77°, two; of 76°, one; of 13°, three; of 11°, one; of 10°, three; of 8°, one; of 6°, one; of 5°, three; of 4°, one; of 2°, one; of 1°, one; and of 0°, one. Obviously, so long as any peroxide is present, the electromotive force is constant; hence this wide variation of current strength must be attributed apparently to the variation of the resistance in the different cells. But this is not the worst aspect of the matter. If the discharge be continued for a longer time, some of the cells become exhausted sooner than others, and are then charged in the inverse direction by the current from the rest. On the 12th of June, the thirty-two cells, after pretty complete discharge, were measured as above. Nineteen cells gave positive deflections ranging from 87° to 5°; thirteen gave negative deflections varying from 13° to 1°. Faraday said of primary batteries in 1834, that "weak and exhausted charges should never be used at the same time with strong and fresh ones in the different cells;" and this remark is even more applicable to secondary batteries, where this condition is more liable to occur. Not only does a dead cell introduce resistance into the circuit, but what is of more importance, its reverse polarization introduces a counter-electromotive force, so that every such cell neutralizes another cell, the dead resistance of which is also added to the circuit. In consequence, it is not safe in practice to draw from a secondary battery all the energy it should be capable of yielding, and in most cases not more than half can be safely taken out of it. In place of continuing uniform as in a single cell, the electromotive force of a series of cells begins to fall when about half the charge which it ought to be capable of yielding has been drawn from it. The practical result is that to supply for lighting a definite number of coulombs of electricity, a secondary battery of twice the size is necessary under these circumstances.

With reference to the two forms of lead secondary battery devised by Planté and by Faure respectively, my experiments show that while the electromotive force is the same in both, and while the internal resistance and storage capacity may readily be made the same, yet that the local action is far less when the peroxide is

formed by the action of the current upon the lead plate itself than when it is produced from minium spread upon it. The rapidity of the formation of the peroxide is proportional to the density of the current; but when rapidly formed it is fine and spongy. In one of my Planté cells, the peroxide is beautifully crystalline and very hard. Not a trace of sulphate has been formed in it apparently, though it has been in use for six months, and has been frequently charged and discharged during that time. The necessarily finer condition of the peroxide in the Faure battery would seem to predispose it to a more considerable local action. True, the Faure battery requires very much less time than the Planté to form it. But this is more than compensated by the increased permanence of the latter. With an equal storage capacity, as in M. de Kabath's form of Planté cell, it would appear that in efficiency it will surpass the form devised by Faure.

The commercial aspect of the question of the storage of electrical energy has recently assumed a very considerable importance. But it is evident at the outset, that an agent, which has been produced and then stored, must cost more when redelivered than when first produced, by exactly the cost of the storage; that is, supposing there is no loss in storing. But there is a loss, and this is almost if not quite one-half of the energy involved; as it would seem from the Conservatoire experiments in Paris, among others. The first cost of the accumulators, the expense of charging them, their low efficiency, and especially their bulk and weight, must ever prevent, it would seem, their competing successfully with the direct use of the dynamo-electric machine, at least with any form of secondary battery yet devised. For special uses, however, the storage battery has a high value. Even if the cost of an ampere of current produced by it is twice as great as if produced directly by the dynamo-machine, this cost is only one-half of that required to give the same current by any available form of primary battery. Where a strong current of low electromotive force is needed, under conditions where a dynamo cannot be employed, there the secondary battery has its most important field. Its value rapidly decreases as the number of cells is multiplied, as when a high electromotive force is needed for the production of light. Its chief advantage is its transportability; since by placing it near the work to be done, the loss of energy on long conductors is obviated.

On modifications of the Spectrum of Sodium vapor in a Bunsen Flame. By Charles S. Hastings, of Baltimore, Md.

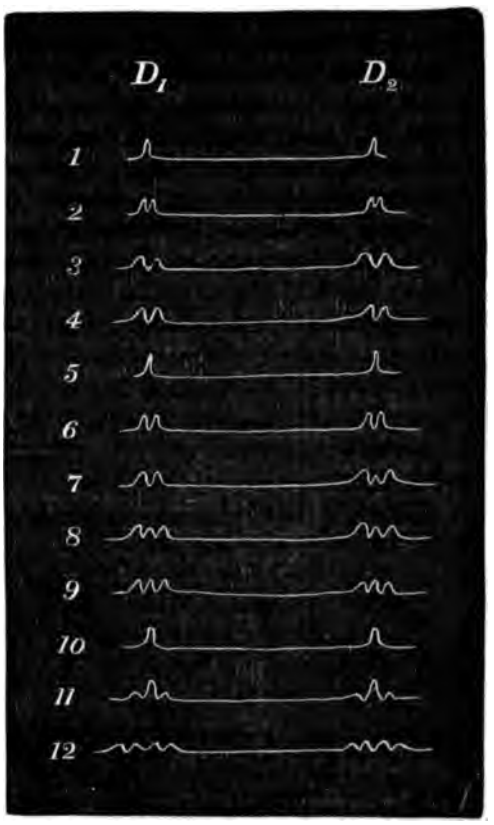
[ABSTRACT.]

At the Boston meeting of the Association, Prof. Young described an experiment, with a sodium-tinted Bunsen flame having a lime-light behind it, which in some respects appeared to offer contradictions to the fundamental theory of spectrum analysis. To an explanation of the phenomenon in accordance with accepted theory which I suggested in a letter, he objected that it implied the existence of phases which he had not observed. To satisfy my own mind I tried a series of experiments shortly after, which presented some striking results, predicted, it is true, by the explanation which I had entertained, but worth noting.

In front of the slit of the large spectroscope belonging to the Johns Hopkins University, was placed a Bunsen burner with its top about two inches below the slit. This gave a flame seven inches in length, hollow for more than half its length and with well defined inner and outer walls. The dispersing member of the spectroscope was a Rutherford grating of over 30,000 lines, and the observations which follow were made in the spectrum of the second order with a magnifying power of 26. This power was quite sufficient to show the duplicity of b_3 and b_4, and even of E_1. Into the flame, a quarter of an inch below the level of the slit, was introduced a bead of fused chloride of sodium; a less volatile compound would not yield the results here described.

When the bead was gradually moved through the flame, starting from the side next the slit, the spectrum underwent a series of changes of which the typical forms are given in the cut from 1 to 10 inclusive. Here the spectral lines are represented by the ordinary convention, abscissas representing deviations and ordinates the corresponding intensities. Thus 1, 5, 10 corresponding to positions where the bead was hardly touched by the flame—5 being in the centre of the hollow portion—represent simply two sharp bright lines; 2, 3, 4, 6 indicate lines more or less widened with a narrower dark central line; 8 and 9 exhibit the lines still wider, so that they may be more properly called bands, striped by two black lines each; 7 is interesting as emphasizing a difference in the two sodium lines which are ordinarily so like. By lowering

the flame until the top of the hollow centre was but just above the slit, a spectrum represented by 11, which may be regarded as a simple modification of 9, was obtained; and if, while in this position, another bead was introduced cautiously into the *side* of the flame and lower down, the phase described by 12 might be observed. This form of the sodium spectrum at low temperature, consisting of eight bright lines defining six dark ones, I have never had any difficulty in reproducing when the pressure on the gas mains was sufficient to give a good flame.



The explanation of these phenomena, though in some cases a little involved, is easy to one who has mastered the fundamental principles of spectroscopy.

Perhaps it can be given in the fewest words by formulating certain laws governing gaseous radiation, viz.:

(*a*) A gas can only produce a dark line by absorption when at a lower temperature than the source of radiation.

(*b*) Even then it can produce a dark line only when it exists in a sufficient abundance.

(*c*) The brightness of a spectral line can be increased in two ways, either by increasing the number of molecules which send the proper vibrations to the objective of the collimator, or by

increasing the temperature of the gas of which the molecules are a part.

(*d*) By increasing the density of a gas, or by increasing its temperature at a constant density (in other words, by increasing the frequency of molecular impact), its spectral lines become broader.

These laws may be applied as follows: When the bead touches the flame at any one point of its four surfaces in line of axis of collimator, only a small amount of sodium is volatilized, and this rare hot vapor gives well defined lines. If the bead is gradually carried into the flame from the hither side the vaporization first goes on more rapidly and the denser gas yields widened lines, until, when the bead is fully enveloped by the flame, the copious evolution of sodium vapor causes it to overflow the limits of the flame and form a layer of cooler vapor between the hot flame and slit. This gives rise to the forms 2, 3, 4, according as the density varies in different portions. The form 6 corresponds to the case when the sodium bead enters the farther surface of the hollow cone within the flame. This, as is well known, is the hottest portion of the flame, though in this case its temperature is lowered over the solid chloride of sodium, the evaporation of the salt contributing to this end more powerfully perhaps than conduction; thus the physical conditions are not essentially different from those which obtained on the nearer side of the hollow cone and 6 should resemble 3 and 4. As soon as the solid is moved beyond this position, however, the conditions are greatly changed, for then there is a layer of gas at an extremely high temperature between the slit and bead, and very near the latter. If this layer is impregnated with a small amount of sodium vapor, we shall have, according to the laws which have been formulated, bright sharp lines superimposed upon the spectrum of the colder vapors beyond. These last, however, may give such a spectrum as 3, whence the forms 7, 8, 9. If the relative importance of the hot layer be increased by lowering the flame until the conical summit of the inner mantle is nearly opposite the slit, form 11 is easily reproduced. Finally, if, with this spectrum in the field of the spectroscope, we introduce a layer of rare and cool vapor between its source and the slit by means of a second bead, the last form can be secured with little difficulty.

SIEMENS' ELECTRICAL DEEP-SEA THERMOMETER. By J. R. BARTLETT, Commander U. S. Navy.

[ABSTRACT.]

THE principle of this new apparatus for obtaining temperatures of sea-water below the surface is based upon the variations in resistance of metals to the passage of an electric current arising from changes of temperature.[1]

The apparatus was constructed by Mr. Siemens of London, for the U. S. Coast Survey and placed on board the steamer "Blake." It consists of 2600 fathoms of double insulated cable, which is wound on a large reel on the upper deck. To the end of this cable is attached a resistance coil, which can be lowered to any depth desired.

The other ends of the insulated cable are brought to a room on the main deck and are attached to a Wheatstone bridge specially arranged for this purpose. Another resistance-coil of the same material and resistance as that lowered into the sea is likewise attached to the bridge. The latter coil has an attached thermometer and is kept in a copper vessel filled with water. A battery of twelve Leclanche's cells and a Thompson's marine galvanometer are attached to the bridge. The resistance-coil at the end of the cable being lowered into the sea just below the surface and some of the same water placed in the copper vessel containing the comparison-coil, the reflected pencil of light, when a current is sent into the bridge, will be at zero on the scale, as under these conditions both resistance-coils are in the same temperature. If the cable is then paid out and the resistance-coil at its end reaches any depth, say 100 fathoms, where the temperature is lower, the pencil of light will be deflected to the left, when the circuit is closed. Now, if cold water is added to that in the copper vessel containing the comparison-coil, with the circuit closed, until the pencil of light comes back to zero on the scale, it is assumed that the water around each coil is the same, and the reading of the attached thermometer of the comparison-coil will give the temperature at 100 fathoms, the depth to which the cable has been lowered. After taking a series of temperatures to 800 fathoms in this way, the cable was reeled in, stopping at intervals for temperatures; in this case, with circuit closed, the deflection of light was to the right, and it was

[1] A diagram, showing arrangement of apparatus, is given in Jour. of American Geographical Society, Vol. XIV, 1882; also in No. 20 of the Proceedings U. S. Naval Institute.

necessary to raise the temperature in the copper vessel until the light was brought to zero on the scale.

Water frozen by a Carré ice machine was used to reduce the temperature of the deck resistance coil.

Trials were made in latitude 36° 50′ N., longitude 74° 25′ W., during a perfect calm, with smooth sea and the ship's engine at rest, under which conditions the results of our several lowerings were very satisfactory. Having taken many thousand deep-sea temperatures with the Miller-Casella thermometers, my confidence in them was greatly strengthened when, after repeated lowerings, their readings agreed with the Siemens apparatus. Readings can be taken with the latter to one-quarter of a degree; but with the Miller-Casella it is difficult to decide on half a degree.

In a previous trial in the strength of the Gulf Stream, off Charleston, during moderate weather, with the vessel rolling 15°, it was found very difficult to read the galvanometer, and this could only be done when the engine was at rest. Under these trying circumstances, however, the temperature agreed with the Miller-Casella. I give an example of a single lowering off the Chesapeake.

	Depth in Fathoms.												
	Surf.	5	10	15	20	30	50	75	100	150	200	300	400
Siemens App..	76½	76½	76½	69	58	54	54½	52½	50½	46½	43½	40½	40
Miller-Casella..	76½	76½	76	68	58	54	53½	52½	50½	46½	43½	40½	40

The above was taken on August 11; on the 12th the Siemens thermometer indicated a warm stratum of water between thirty and seventy-five fathoms; the Miller-Casella gave only the maximum and minimum, and did not indicate this. The temperature at twenty fathoms was 57° with both thermometers, at thirty, 52°; but at fifty fathoms the Siemens thermometer read 54½°, while the minimum of the Miller-Casella read the same as at thirty fathoms; at seventy-five fathoms the Siemens read 53°, the Miller-Casella still 52°; at 100 fathoms they agreed at 50½°. These observations were continued during the entire day with always the same results.

By lowering the Miller-Casella thermometer at the rate of 200 fathoms per minute to 50 fathoms, and hauling it by the 30 fathom depth at the same rate of speed, the temperature would read as high as 53½°.

An Instrument for readily producing Low Temperatures. By Henry Carmichael, of Brunswick, Me.

[ABSTRACT.]

The instrument exhibited consists substantially of three test tubes placed within one another.

The outer tube is four centimetres wide and thirty-one centimetres long. The middle tube is of the same length and two and a half centimetres wide. Upon it have been blown a series of enlargements for increasing the surface, which, however, do not prevent a free passage of air between the tubes. The bottom of the latter tube contains numerous perforations which have been made by plunging a red-hot needle through the glass already softened before a blast lamp. This tube is held in place by a rubber ring which makes a tight joint at the top, and by a suitable aperture admits an air current to the annular space between the tubes. The bottoms of the tubes are one-half a centimetre apart.

The third tube is somewhat longer than the others and one centimetre wide. It is intended to hold the substance to be submitted to a low temperature.

For producing low temperatures, liquids are used which in a current of air blown through them are rapidly vaporized.

The operation of the instrument is as follows: 50 c c. of the volatile liquid is introduced in the middle tube. The aperture in the rubber ring is connected by flexible tubing with bellows, and the air-current thus introduced at the top of the narrow annular space between the outer and middle tubes flows to the bottom and there passing through the fine perforations beats the liquid into foam.

If ether or the petroleum distillate "gasolene" is used, the liquid is quickly reduced to —23°C. Six grammes of water placed in the inner tube is speedily frozen.

In conducting this experiment to save the tube, it is important to withdraw it before the water is congealed to the centre. The ice cylinder is easily shaken out of the tube after warming it in the hand.

By substituting more volatile liquids in the evaporator, lower temperatures are reached. Seventy-five grammes of mercury are almost instantly solidified in a bath of sulphurous anhydride.

When the vapors discharged are undesirable in the work-room,

they are conveyed away through flexible tubing connected with an aperture in a cork ring which forms there a joint between the middle and inner tubes.

The effectiveness of this apparatus depends largely upon the fact that the escaping cold saturated current is made to cool the warm entering air before it comes in contact with the liquid to be evaporated.

Whatever moisture is contained in the air is deposited as frost upon the middle tube, and when the transparency of the latter is to be preserved the air is previously dried.

The instrument described is not merely for illustrating the fact that evaporation is accompanied by an absorption of heat, but is intended as a convenient means of securing for a short or long period the low temperatures necessary in many physical and chemical researches.

By a slight change in its construction, the perforations are placed above the bottom, and the region of intense cold remains free from the foam that might otherwise interfere with the direct observation of phenomena at the bottom of the middle tube.

Upon a proposed method of producing Artificial Respiration by means of a Vacuum Jacket. By Alexander Graham Bell, of Washington, D. C.

[ABSTRACT.]

I PROPOSE to surround the waist of the unconscious patient by a rigid jacket or drum somewhat larger in diameter than his body. The apparatus can be rendered practically air-tight by a rubber band round the thorax, and another round the loins. Upon exhausting the air inside the drum a partial vacuum is produced around the abdomen. Under such circumstances the pressure of the atmosphere forces air through the mouth and nose into the thorax causing the depression of the diaphragm and consequent expansion of the abdomen.

The alternate rarefaction and condensation of the air confined around the abdomen thus cause alternate inspiration and expiration.

RADIANT HEAT AN EXCEPTION TO THE SECOND LAW OF THERMODYNAMICS. By H. T. EDDY, of Cincinnati, O.

[ABSTRACT.]

THIS paper attempts to show that the second law of thermodynamics must receive some modification in the case of radiant heat, by reason of the fact that heat is not propagated through space instantaneously.

TITLES OF OTHER PAPERS READ IN SECTION B.

DANGERS FROM LIGHTNING INCREASED BY TELEGRAPH WIRES. By Stephen S. Haight, of West Farms, N. J.

ATOMIC PHYLLOTAXY. By Pliny Earl Chase, of Philadelphia, Pa.

THE TENSION OF THE LUMINIFEROUS ETHER. By De Volson Wood.

THE FORCE OF DIFFUSION OF THE GASES FORMING THE ATMOSPHERE. By George Iles, of Montreal, Canada.

*THE COLOR OF THE SUN. By S. P. Langley, of Alleghany, Pa.

*ELECTRIC INDUCTION BY STRESS. By S. W. Robinson, of Columbus, Ohio.

*A NEW SOIL-THERMOMETER. By D. P. Penhallow, of Mountainville, N. Y.

*ON TEMPERATURE OF THE SUN. By A. E. Dolbear, of College Hill, Mass.

ON THE TELEPHONE AS AN EXPLORER IN AN ELECTRIC FIELD. By A. E. Dolbear, of College Hill, Mass.

*ON THE CHARACTER OF HEAT. By A. E. Dolbear, of College Hill, Mass.

*The papers thus marked were only read by title owing to the absence of the authors when the papers were called.

ADDRESS

BY

H. CARRINGTON BOLTON,

VICE PRESIDENT, SECTION C.

FELLOW MEMBERS OF THE CHEMICAL SECTION;

Ladies and Gentlemen:—

THE recognition by the Association of the equal rights of chemical science and the elevation of the late "Permanent Subsection" to the dignity of a "Section of Chemistry," now assembled for the first time, mark an important epoch in the annals of our organization. Permit me to offer congratulations on our promotion and to express my high appreciation of the honor of presiding over your deliberations.

The Permanent Subsection of Chemistry was organized at the Hartford meeting of the Association in 1874. It had its origin in the action of a number of chemists assembled at Northumberland in August of the same year to commemorate Priestley's discovery of Oxygen. At that memorable gathering a discussion of the advantages of forming an independent organization in the interests of chemistry led to the appointment of a committee* "to cooperate with the American Association for the Advancement of Science at their next meeting, to the end of establishing a chemical section on a firmer basis."

This committee met at Hartford a few weeks later and united with the chemical members of the Association in founding a Per-

*The committee consisted of Profs. Silliman, Smith, Horsford, Hunt and Bolton.

manent Subsection in accordance with the provisions of the new constitution adopted at the same meeting. Meetings of the subsection were held on two days and arrangements were made for its permanent organization. In the following year the Association met at Detroit and the Subsection re-assembled under the chairmanship of Prof. S. W. Johnson. Before adjourning the section elected Prof. Geo. F. Barker chairman for the ensuing year and passed a resolution requesting the chairman-elect to prepare an address. This was the origin of the custom which it is my pleasant duty to follow. Professor Barker accepted the task and his masterly essay on the Atom and the Molecule (1876) is remembered by many in this audience. Since then Prof. F. W. Clarke (1878) has urged in your hearing the endowment of laboratories of research; Prof. Ira Remsen (1879) has magnified the claims of his chosen field of investigation — organic chemistry; and Prof. J. M. Ordway (1880) has gracefully surveyed the experiences of the past, the needs of the present and the prospects of the future of chemistry.

The amendments to the Constitution, adopted by the Association at the Cincinnati meeting, provide, as you are aware, for nine sections, each with its own presiding officer; since this may involve an equal number of annual addresses, it is hardly to be expected that each will maintain the elaborate character of those of former years.

I ask you therefore to bear this fact in mind while I invite your attention to a rather superficial survey of

CHEMICAL LITERATURE.

The literature of chemistry, extending as it does through a period of more than fourteen centuries, varies greatly in character, in province and in design; it partakes of the peculiar phases exhibited by the science at different epochs and depicts the experiences and thoughts of those who cultivated it in all ages. It may be studied from several points of view: the biographer searches the voluminous records to acquire knowledge of the intellectual activity of individuals; the historian unfolds the progress made by the science in a special field or in its entirety, with philosophical inquiries respecting effects and causes; the bibliographer, scarcely penetrating beyond the title pages of the dusty tomes, laboriously catalogues them to facilitate the researches of others.

We do not propose to give you a biographical, an historical or a bibliographical treatise, but rather to review chemical writings as sources of information and as portions of the world's literary productions. We shall concern ourselves less with the questions what were the personal history and life-work of a given author, and more with the queries what are the characteristics of the various classes of works at different epochs, what discoveries do they chronicle and what was their influence on the contemporaneous science.

The very earliest information concerning chemical arts comes to us from that ancient nation supposed by some to have given its own name to the science itself; not only do the sculptured tombs and temples of Egypt portray with unimpeachable authenticity and wonderful accuracy the technical skill of that venerable people, but these same monuments are even now relinquishing their hold on long-buried treasures in the form of papyri, whose perplexing script no longer conceals their meaning from the erudition of Egyptologists.

Of these miraculously preserved papyri the most valuable to chemistry is that discovered by Prof. George Ebers at Thebes in 1872, and named after its learned discoverer. We have described this elsewhere[1] and shall not here enter into details. It is the most ancient medical work extant, being assigned to the sixteenth century B. C., and contains a vast amount of information on the medical practice and the pharmaceutical preparations at that remote period. The unknown author wrote less obscurely than many of a much later date, and when the whole papyrus shall have been deciphered it will prove an invaluable contribution to chemical history.

The most ancient manuscript treating exclusively of chemical operations is a Greek papyrus of Egyptian origin preserved in the Library of the University of Leyden. Its authorship is unknown, its date is placed by Reuvens in the third or fourth century A. D. This MS. consists of a collection of prescriptions and receipts for conducting various operations in metallic chemistry, such as the testing of gold and silver; the purification of lead, of tin and of silver; the hardening of tin and of silver; the albification of copper, etc. It deals little with alchemy though some of the receipts evidently refer to transmutations, as those entitled: "the prepara-

tion (artificial?) of silver;" "the preparation of gold;" "the purification of tin by silver," etc.

Reference is made to sandarach (realgar), cadmia (zinc ore), chrysocolla, cinnabar, natron (soda), mercury and other chemical substances, but no receipts are given for their preparation. The author quotes from the Materia Medica of Dioscorides who probably preceded him by about two centuries. It is to be regretted that the full text of this ancient manuscript has never been published; the little known of it foreshadows information of great interest.[2]

The great libraries of Paris, Rome, Venice, Milan, Escurial, Cracow, Gotha, Munich and Cologne preserve a large number of Greek alchemical manuscripts of unknown authorship and uncertain date. Hoefer, the French historian of chemistry, refers them to the third and fourth centuries,[3] but other authorities with greater probability place them not earlier than the tenth and eleventh.[4]

The most celebrated of these essays are attributed to Zosimus, of whose history nothing is certainly known, and bear these titles: "On Furnaces and Chemical Instruments," "On the Virtue and Composition of Waters," "On the Holy Water," "On the Sacred Art of Making Gold and Silver." In a treatise attributed to Synesius, we find a description of a hydroscopium or hydrometer which was rediscovered as long after as the sixteenth century.

In a treatise attributed to Olympiodorus, he cites as authorities Democritus, Anaximander, Zosimus, Pelagius, and Marie a certain Jewess whom the later alchemists confounded with Miriam, Moses' sister.

In these manuscripts chemistry is called the "sacred art" and the exceedingly obscure and figurative language in which they are written makes it well nigh impossible to separate fact from fancy; Hoefer has indeed attempted to discover modern chemical conceptions in the allusions to Egyptian myths and the chaotic collections of spagyric arcana.

Of systematic nomenclature there is absolutely no trace; indeed each author seems to have aimed to write treatises intelligible only to himself, and we greatly doubt his success in even this respect. "Calmia," we are informed, "is magnesia," and "magnesia is the female antimony of Macedonia," "mercury is white sulphur which produces brass;" equally clear is the statement that the

"apospermatism of the dragon is the mercury of cinabarn." That lexicons were early in demand is not surprising; in fact some of the most ancient MSS. are "vocabularies of the sacred art," but even with their assistance it is difficult to form satisfactory concepts of contemporary chemical science.

Suidas,[5] a Greek lexicographer of the eleventh century, states that Diocletian having conquered the rebellious Egyptians (296 A. D.) destroyed their books on the preparation of silver and gold, lest becoming rich by the practice of that art they might again resist the Romans. Regrets at the wanton acts of this imperial biblioclast are tempered by the reflection that modern scholars are spared the study of such literary absurdities.

The Chinese, that curious people who always claim a hearing when the origin or antiquity of arts and sciences is under consideration, were acquainted at a very remote period with many branches of chemical technology. We do not know of any special chemical literature produced by them, but the researches of Rev. Joseph Edkins[6] and of Dr. W. A. P. Martin[7] make it highly probable that scholars will yet discover contributions of no small importance to the early history of chemistry. Prof. George Gladstone[8] has endeavored to show that the Chinese originated the doctrines and pursuit of alchemy and communicated it to the Arabians by whom it was disseminated throughout Europe.

The high state of civilization and extraordinary intellectual development of the Arabians has left a deep impression on chemical science. Cultivated chiefly by physicians, attention was directed to its pharmaceutical applications, and in spite of the prohibitions of the Koran to the fascinations of alchemy. Of their extant writings, preserved in European libraries, only a portion have been edited; those best known partake of the poetical imagery and hyperbole characteristic of the Oriental mind. This is shown to some extent in the singular titles prefixed to their treatises, *e. g.*, "The Rise of the Moon under the Auspices of Golden Particles," by the alchemist Dschildegi; "A poem in the Praise of God, of Mahomet and of Alchemy," by Dul-nun-el-Misri.[9]

The well known treatises of Geber,[10] "Of the Investigation of Perfection," "Of the Sum of Perfection," "Of the Invention of Verity," and "Of Furnaces," notwithstanding a bewildering style of composition, which seems to confirm Dr. Johnson's derivation of

gibberish, from Geber, display very great familiarity with a large number of chemical substances and operations.

Geber's works are generally assigned to the eighth century and consist chiefly of compilations from the "Books of the Ancients;" he mentions no author by name. They contain chapters devoted to the seven known metals, to the methods of distillation, calcination, cupellation and other operations, to the preparation of saline substances and to chemical philosophy. Geber adopted Aristotle's views of the constitution of matter from four principles, the hot and cold, the wet and the dry, and adds thereto: "Mercury and sulphur are the components of metals," a doctrine which with slight modifications prevailed for more than eight centuries. Geber describes the preparation of nitric acid, of aqua regia, and of mercuric oxide; he mentions the increase in weight of metals when calcined with sulphur, and gives the results of a rude quantitative analysis of crude sulphur. He constantly maintains the doctrine of transmutation of metals and gives a refutation of the ingenious arguments opposed thereto. His remarks on the qualifications of a chemist are most intelligent and are not inopportune in modern times; he urges the necessity of diligence, patience, learning, a temperate disposition, slowness to anger, and a full purse, "for this science agrees not well with a man poor and indigent," together with faith in the God who "withholds or gives to whom he will" the secrets of nature, and who will infallibly punish the foolish meddler with magical mysteries.

To detail fully our obligations to Arabian chemists is no part of our plan. They have left an indelible impression on the very language of the science, in the words alcohol, alembic, alkali, borax, and many others. All honor to the intelligent authors who a thousand years ago defined chemistry as the "Science of Combustion, the Science of Weight, the Science of the Balance!" [1]

In the middle ages intellectual activity was confined largely to the clergy, who controlled the schools of learning, the libraries, and nearly all sources of knowledge. University chairs were occupied exclusively by clerical professors[2], literature and science were cast in ecclesiastical moulds. Scientific treatises were the production of monks and emanated from cloisters. Many distinguished philosophers mastered widely separated branches of learning: among these were Alain de Lille (b. 1114), celebrated as a

physician, theologian, poet and historian, who filled the episcopal chair at Auxerre; Roger Bacon (b. 1214) an English cordelier; Raymond Lully (b. 1235), a Franciscan friar, and Albertus Magnus (b. 1193), Bishop of Ratisbon. The latter, amid the monotonous routine of a Dominican monastery, found leisure to distinguish himself in astronomy, medicine, alchemy and, according to his enemies, in necromancy. At this remote period, accusations of dealing with magic were not unfrequently made against those whose learning and skill in experimental sciences excited envy and superstitious zeal.[13]

To treat the writings of these eminent ecclesiastics as a part of chemical literature requires perhaps a stretch of the imagination, yet three hundred years ago they were regarded as masterpieces of the science and formed the text-books of students of alchemy. The writings of these ecclesiastical philosophers are as comprehensive as the branches of learning they cultivated, and incredibly voluminous; Albertus Magnus' collected works fill twenty-one folio volumes.[14] But a small fraction of these treatises are occupied with science and chemistry; and of this fraction there is in many cases a reasonable doubt as to their authenticity. In fact, nothing was more common than the ascription of work by an obscure second-rate writer to some celebrated philosopher of preceding ages, in order to give the work the stamp of authority,—a deception which previous to the invention of printing was more readily accomplished.

It became difficult therefore to distinguish the apocryphal writings from the genuine. The former, it is true, frequently betray themselves by anachronisms and other blunders, but many ingenious writers avoided such traps by adopting an enigmatical style worthy of the Delphian oracles.

Basil Valentine was the reputed author of works held in the very highest esteem by the alchemists of the Middle Ages, yet the very existence of this individual is seriously questioned. Mystery surrounds Valentine's entire history, and his writings were given to the world in a most dramatic manner; according to tradition they were hidden in the wall of a church at Erfurt and long after his death a thunderbolt shattered the wall and revealed the precious documents.

Whether Valentine was a real personage or not the works ascribed to him exhibit great familiarity with many chemical sub-

stances and operations, though the obscure and incoherent style renders their intelligent perusal very difficult.

Valentine's celebrated "Chariot of Antimony,"[15] extolling the medical virtues of this metal, is perhaps the least obscure of his works; the "Twelve Keys of Philosophy"[16] with its singular plates, one of the most unintelligible; yet beneath the extravagant jargon characteristic of the period, glimpses are obtained of light and intelligence. The latter work presents clearly the theory that all metals are compounded of three principles: fixedness, metallicity and volatility, represented respectively by salt, mercury and sulphur, an hypothesis which long completely controlled chemistry until it gave place to the seductive theory of Phlogiston. It is uncertain whether the works ascribed to Valentine were first written in Latin or in German; his writings were collected in the seventeenth century and have been through many editions.[17] Several of his treatises have been translated into English and into French.[18]

In the fifteenth century the newly invented printing press was employed in the production of few works which can be regarded as chemical, and these were chiefly confined to isolated treatises of the ancient philosophers; in the sixteenth century the alchemists began to publish the results of their industry and speculations, and in the succeeding century a prodigious number of alchemical works were issued in Germany, France and England, creating literature of an extraordinary type.

Some of these treatises, which are numbered by thousands, record valuable experiments made by enthusiasts seeking the philosopher's stone, but the majority contain "a crude mass of incoherent propositions and wild assertions, a mixture of poesy and insanity, in which all logical ideas are lost amidst the stilted phraseology, but through which breathed a blind yet fervent faith."[19] Great obscurity of style[20]; an enigmatical method of naming chemical substances which found its utmost development in the use of arbitrary symbols and hieroglyphics, representations of alchemical processes[21]; the intimate association with astrology, the boast or affected [illegible][22]; the extravagant [illegible][23]; the allusions to [illegible] and to the mysteries of Hermes [illegible]—[illegible]

ical history as they are profitless to the practical worker in modern science.

Among the fabulous writings, highly esteemed by the credulous alchemists, may be mentioned the celebrated inscription of Hermes Trismegistus upon an Emerald Tablet[25], the Golden Leaves of Abraham, Jew Prince, Priest, Levite, Astrologer and Philosopher, which in the hands of Nicolas Flamel[26] yielded such a rich harvest, the Practical Chemistry of Miriam the sister of Moses[27], and a multitude of grotesque writings ascribed to personages of known reputation. Raymond Lully is credited with five hundred works; Hermes Trismegistus, the mythical Father of Sciences, with several thousand.

Many of the alchemical works published during the period of which we speak are degraded by admixture of the contemporaneous pseudo-sciences, judicial astrology and magic. To this class belong the celebrated works of Dr. Fludd and the writings of the Rosicrucians; excluding these as wholly beneath our consideration the number of occult works on alchemy is still very large. We imagine it will be hard to discover in the whole range of literature writings having scientific pretensions more senseless than the aphorisms of the disciples of Pythagoras, collected in the "Turba Philosophorum"[28] so often quoted by the alchemists of the sixteenth and seventeenth centuries. Its improbable character is perhaps equalled by the "Gloria Mundi" in which the anonymous author favors his readers with the chemical views of Aristotle, Plato, Socrates and Democritus, interspersed with equally authentic statements by Hermes and Morien, Lamech and Methuselah, Abel and Seth, and even of Adam himself.[29]

In the early part of the seventeenth century, Michæl Maier, physician to the Emperor of Germany, Rudolph II, a royal patron of astrologers and alchemists, published several treatises now much sought after by alchemical bibliophiles.[30] Maier's "Symbola aurea mensæ" and "Atalanta Fugiens" contain emblematic plates, supposed to illustrate the hermetic interpretation of the fables and allegories of Egypt and Greece.

The connection between these ancient mythologies and the secrets of the philosopher's stone was a favorite subject with many authors and has been exhaustively treated by the Abbé Pernety in his two curious works "Fables Egyptiennes et Grecques dévoilées" and "Dictionnaire Mytho-hermétique."[31] Not content with prose, several authors clothed their alchemical inspirations in poetry;

of these may be mentioned the Arabian treatises already named, the Twelve Gates of Alchemy by Sir George Ripley, written about 1450[32], the Crede Mihi of Thomas Norton, written about 1477[33] and the Chrysopœia of Aurelius Augurelli.[34] The latter written in Latin hexameters with more pretensions to elegance than usual with the prosaic alchemists was dedicated in 1514 to Pope Leo X. Leo rewarded Augurelli by presenting him with an empty wallet, remarking that one who knew so well how to make gold had need only of a purse.

The alchemists, in common with their contemporaries in other branches of literature, took pleasure in prefixing to their essays eccentric titles; Helvetius writes of "The Brief of the Golden Calf (The World's Idol) discovering the rarest Miracle in Nature;"[35] Glauber names one of his treatises, "The Golden Ass well managed and Midas restored to reason."[36]

Perhaps the height of absurdity is reached in the famous *Liber Mutus*[37], which consists of a series of fifteen symbolical engravings purporting to disclose the whole Hermetic Philosophy. The utterly unintelligible character of much alchemical literature is occasionally acknowledged by those who otherwise accepted the prevailing popular belief in transmutation; Lenglet du Fresnoy, speaking of Raymond Lully's Clavicula, says "Lully assures us that this treatise is indispensable to the comprehension of his writings; but, after reading it, one is little wiser than before."[38]

There are several large collections of alchemical treatises which the curious in these matters should consult; the most extensive is Zetzner's Theatrum Chemicum[39] published in 1613 in six octavo volumes; this contains no less than 209 distinct treatises, notwithstanding which Lenglet du Fresnoy remarks that many excellent ones are wanting. Manget's Bibliotheca Chemica Curiosa,[40] published in 1702 in two folio volumes, contains 133 treatises; and the Museum Hermeticum[41] (1678) contains 21 treatises, some of which are illustrated. The most extensive English collection is Ashmole's Theatrum Chimicum Britannicum, published in 1652, and a noted French collection is Salmon's Bibliothèque des Philosophes Chimiques (1672), of which the edition by Ricchebourg (1741) is by far the best.[42]

Stimulated by avaricious hopes, the zealous alchemists labored most industriously; and, by subjecting mixtures of all known substances to heat and to the action of air, discovered a multitude of new bodies having more or less medical and economic value.

As we have seen, their writings consist for the most part of monographs describing disconnected experiments, with no attempt at exhaustive treatment of any single topic, no classification of phenomena and no well studied arrangement of material such as characterizes later handbooks. Nor can a scientific collocation be expected during the formative period of chemistry and previous to the introduction of theories around which to group the isolated phenomena. One of the earliest attempts to treat chemical facts in a systematic manner was made by Sir George Ripley, Canon of Bridlington, who lived in the fifteenth century. In his "Compound of Alchemy" written in 1471 the whole science of hermetic chemistry is unfolded in a poem divided into twelve sections called "Gates" through which the reader is conducted to the mysteries of transmutation.[43]

"But into chapters thys Treatis I shall devyde,
In numbre twelve, with dew recapytulatyon;
Superfluous rehearsalls I lay asyde,
Intendyng only to give trew informatyon
Both of the theoryke and practicall operatyon:
That by my wrytyng who so wyll guyded be,
Of hys intente perfyctly speed shall he.

The fyrst chapter shall be of natural Calcination;
The second of Dyssolution, secret and phylosophycall
The third of our elementall Separation;
The fourth of Conjunction matrimonial;
The fyfth of Putrefaction then followe shall;
Of Congelation Albyficative shall be the sixt,
Then of Cybation, the seaventh shall follow next.

The secret of our Sublymation the eyght shall show;
The nynth shall be of Fermentatyon;
The tenth of our Exaltation I trow.
The elevent of our mervelose Multiplycatyon,
The twelfth of Projection; then Recapitulatyon,
And so this treatise shall take an end,
By the help of God, as I entend.

. . . . . .

Thus here the Tract of Alchemie doth end;
Which tract was by George Ripley, Chanon, penn'd
It was composed, writt and signed his owne,
In anno twice seaven hundred seaventy one.
Reader, assist him, make it thy desire,
That after life he may have gentle fire! Amen.

Ripley's versification and theme remind one of Chaucer's Canon's Yeoman's Tale composed nearly one hundred years earlier.

We can only briefly allude to Paracelsus' bombastic productions[44], which are concerned chiefly with medical chemistry, and to the remarkable works of George Agricola[45], distinguished by profuse illustration, portraying the apparatus and operations of mining and metallurgy in the sixteenth century.

Conspicuous for accuracy of description and systematic arrangement of topics is the "Alchymia" of Andrew Libavius, published at Frankfurt in 1595.[46] Libavius, a physician and teacher in the gymnasium at Coburg, rejected the absurd doctrines of the adherents of Paracelsus, combated superstition and quackery and, excelling in observation of chemical phenomena, gained a worthy position among his compeers. In his Alchymia he treats of the *Kuchería* or manual operations of chemistry and of the *Chymia* or descriptions of substances, in separate books. The former he divides into two sections, one dealing with the instruments and the other with the management of fires and construction of furnaces. He describes at length a sumptuous laboratory provided not only with every requisite for chemical experimentation, but also with means of entertaining visiting guests, including such luxuries as baths, enclosed corridors for exercise in inclement weather, and a well stocked wine-cellar.

Libavius was the discoverer of stannic chloride which still bears the name "fuming liquor of Libavius"; he describes the method of preparing artificial gems by coloring glass with divers metallic oxides, and he seems to have been the first to apply the balance to the examination of mineral waters. Notwithstanding his progress we [illegible] devotes eighty pages of his Commentary to the Philosopher's Stone in which he was a firm believer. The second [illegible] of his work published in 1606 forms a [illegible] of 80 pages. It has been [illegible] of the First Textbook of Chemistry.

The [illegible] "Chymische" [illegible] Basil Valentine. Although [illegible] Kirchringius Nicolai Primi, a less pretentious work, is [illegible] to be [illegible] and [illegible] as well as [illegible] [illegible] [illegible]

and arranged them in a logical, systematic manner. The first six chapters treat of the theory of chemistry; the author admits five elementary principles: phlegm (or water), spirit (or mercury), sulphur, salt and earth, the first three being ingredients of volatile substances, and the latter those of refractory bodies. He defines metals as hard bodies generated in the bowels of the earth, capable of extension under the hammer and of being melted by fire. He divides metals into the perfect and the imperfect, and also into male and female, the latter subdivision being based upon their behavior with acids; gold, lead and antimony are male metals because soluble only in aqua regia, and the other five, silver, copper, iron, tin and mercury are female, because soluble in unmixed acids.

In the second part the author discusses the manipulations of chemistry and recognizes many nice distinctions now overlooked; for example, under the head of mechanical division he describes in detail the following operations: "limation, rasion, pulverization, alcoholization, incision, granulation, lamination, putrefaction, fermentation, maceration, fumigation both dry and humid, cohobation, precipitation, amalgamation, distillation, rectification, sublimation, calcination both actual and potential, vitrification, projection, lapidification, extinction, fusion, liquation, cementation, stratification, reverberation, fulmination or detonation, extraction, expression, incineration, exhalation, digestion, evaporation, desiccation, circulation, congelation, crystallization, fixation, volatilization, spiritualization, corporification, mortification and revivification."

In the experimental part Le Febvre explains the arrangement in the following language:

"We shall begin with the meteoric bodies, rain, dew, honey, wax and manna; we shall then describe the preparations made from animals and their secretions; next, the numerous products of the vegetable world; and lastly, the mineral kingdom with its stones, salts, marcasites and metals;" a division still recognized.

The order in which he treats mineral chemistry is as follows: Earths; stones, precious and otherwise; metals; semi-metals, embracing mercury, antimony and bismuth; salts, including common salt, saltpetre, alum, sal-ammoniac and the vitriols; and lastly, the sulphuretted minerals, arsenic, etc. Under each division he gives precise instructions for numerous experiments upon these

substances and describes their medicinal value. The whole work is written in a clear style, free from affectation of mystery; it rapidly passed through five editions, and was translated into English and German.

Three years after the appearance of Le Febvre's Traité, his successor at the Jardin des Plantes, Christopher Glaser[49] of Basle, published a work having the same title also marked by special excellence.

The most successful handbook of the seventeenth century was undoubtedly the "Cours de Chymie" by Nicolas Lemery,[50] an eminent lecturer in Paris. The first edition of this work was published in 1675, and it reached the tenth edition before the close of the century; a fourteenth edition enlarged by Baron appearing as late as 1756. It was also translated into English (1677), German (1698), Italian (1763), and Spanish. The remarkable success of this work is due to a facility for describing dry facts with remarkable simplicity and accuracy. His style is more concise than Le Febvre's, and his arrangement of material shows a progressive spirit. The limits of this address forbid an analysis of Lemery's Handbook, which, moreover, is better known than some others to which we have granted fuller treatment.

Passing over a period of fifty years, the next complete compendium of chemical knowledge which we notice is the "Elementa Chemiæ" of Hermann Boerhaave, the celebrated professor of botany, chemistry and medicine in the University of Leyden. Boerhaave's great erudition, purity of style and brilliant eloquence attracted students in great numbers; such was the popularity of his lectures that "certain booksellers who aimed at lucre by the most scandalous means" published them in 1724, without his authority or consent; this "surreptitious edition"[51] contained, as Boerhaave himself complains, such "false, ridiculous and absurd things in every page," that he was compelled to publish his lectures in an accurate and complete form. Both editions were translated into English by Dr. Peter Shaw, and the authorized work passed through many editions in Latin, French and German.[52] Instead of attempting to give an analysis of two quarto volumes, we will quote Boerhaave's own account of his plan.

"My design," he says, "is to initiate students in the knowledge of chemistry; and to do this in the most effectual manner, I

shall give a clear methodical explication of all that is necessary for understanding the best authors and for performing the chief operations in this experimental art." After acknowledging the difficulty of a systematic treatment of "a science which has been cultivated rather by experiments at random than upon any regular principles, and by persons usually destitute of all . . . knowledge in the liberal arts;" he claims that "these obstacles may be surmounted by making a collection of the several effects, which the art has actually produced, justly deducing general rules therefrom and duly digesting the whole." He then explains the division of his work into three parts.

"The first will rehearse the origin, progress, cultivation and fortune of chemistry, . . ; the second part will deliver certain theorems or principles of chemistry; the third will exhibit the actual operations of chemistry, whereby bodies are changed agreeably to the rules of the art and to the end proposed therein." Boerhaave's sketch of the history of chemistry begins with the earliest times but seems to have been left incomplete; it is rendered useful by classified lists of chemical writings and numerous references.

In his treatment of the theory he departs somewhat from his plan and introduces material which really belongs to the third or practical part. Boerhaave introduces sections on the use of chemistry in natural philosophy, in medicine and in the mechanical arts; gives an exhaustive account of the wonderful nature of fire and experiments in heat, together with ingenious speculations concerning the heat of celestial bodies; he describes the various forms of chemical apparatus and the preparation of curious and useful substances from the vegetable, animal and mineral kingdoms. Boerhaave exhibited the spirit of a true philosopher and produced a work of extraordinary merit. He was quite free from the follies of alchemy, though he cautiously remarks that "we should always remember the limits of nature are by no means to be defined by us, things are taken for impossible which are only unknown by the ignorant" and "many things in chemistry are apparently more incredible than that lead should lose its natural form and be converted into gold." [53]

Contemporary with the systematic compilations of Libavius, Le Febvre, Boerhaave and others, were published hosts of independent works setting forth the results of prodigious labor in the chemical laboratory; many of these are filled with descriptions of

experiments made at haphazard, with no definite object in view, and having no necessary connection with each other.

Glauber's[54] voluminous writings, published in 1658, may be taken as an example of this class; in his works, amid controversies with Galenical physicians and curious apologies resulting therefrom, amid descriptions of alchemical mysteries and receipts for a universal panacea, amid extravagant praises of the "sal mirabile" (Glauber's salt), and curious narratives of personal history, we find many novel chemical facts having a medical or an industrial value. The whole is clothed in a very crude style with an affectation of secrecy and under capricious captions.

On the other hand, some of these treatises show signs of genius, especially in attempts to establish theories in explanation of the familiar yet astonishing phenomena; thus Van Helmont, whose contributions to medical chemistry we pass by, invented the word *gas* to aid in discriminating aeriform bodies[55]; and Rey, in his "Essays on the increase in weight of tin and lead when calcined[56]," demonstrated the weight of the air thirty years before the masterly researches of Boyle.

To attempt in this rapid review to do justice to the philosophical writings of the Hon. Robert Boyle is a hopeless undertaking. From his first publication, in 1660, "New experiments, physico-mechanical, touching the spring of the air and its effects," to his posthumous treatise "Medicinal Experiments," published in 1692, his works teem with ingenious experiments described with great candor and fidelity, and from which conclusions are drawn with cautious reserve and philosophical soundness, as admirable as his rare modesty.

In his "Sceptical Chymist" (London, 1661), Boyle raises doubts as to the elementary nature of earth, air, fire and water, as well as of the alchemical principles, sulphur, salt and mercury, and claims that the first elements of bodies are atoms of different shapes and sizes, the union of which forms what are vulgarly called elements; he argues that their number should not be confined to three nor four, but that probably a large number of primary constituents would eventually be separated or isolated as such. He also first clearly recognizes the distinction between mixture and chemical combination.

The rejection of Boyle's prophetic hypothesis by his contemporaries much retarded the progress of the science. "More, how-

ever, than for his views on the nature of the elements, science is indebted to Boyle for his clear statement of the value of scientific investigations for its own sake, altogether independent of any application for the purposes either of the alchemist or of the physician . . Boyle was in fact the first true scientific chemist, and with him we may date the commencement of a new era for our science when the highest aim of chemical research was acknowledged to be the simple advancement of natural knowledge." (Roscoe.)

Boyle's voluminous writings were collected by Birch in five folio volumes and published in 1744. His diffuse style of composition, with frequent long digressions, renders a perusal of his writings wearisome and led to an edition by Dr. Shaw, in which his works are "abridged, methodized and disposed under general heads," forming three quarto volumes bearing the date 1725.[57] Many of Boyle's papers were published in the Philosophical Transactions.

The literature pertaining to the theory of phlogiston belongs almost wholly to the eighteenth century. It is true that Becher's Physica Subterranea was published in 1659[58], but the theory of a combustible principle existing in all metals remained dormant until Becher's admirer, George Ernest Stahl, published an edition of the Physica Subterranea in 1702; to this he prefixed a long preface elaborating those doctrines which exerted such immense influence on both the theoretical and practical science, for more than a century. The writings of Becher and Stahl are notorious for their barbarous mixture of poor Latin and German, for which indeed both authors apologize.[59]

In Mayow's Treatise on the nitro-aërial spirit[60], we find records of admirable experiments in pneumatic chemistry supported by accurate reasoning and almost prophetic insight of later theories of combustion, but his early death prevented the continued support of his views, and Mayow's treatise was soon forgotten by the prejudiced followers of Stahl.

In the year 1683 a noteworthy event occurred in the chemical world, the opening of public laboratories of instruction. Chairs of chemistry had long existed in institutions of learning, the first being filled nearly eighty years before by Johann Hartmann, and the honor belonging to the University of Marburg. Practical in-

struction had been secured also in the private laboratories of the wealthy, thus Homberg and Friedrich Hofmann worked in Boyle's establishment[61]; but the first public laboratory for instruction was opened at Altdorf, Bavaria, under the direction of Prof. J. M. Hofmann. A survey of the status of chemical knowledge at that date offers a tempting digression but the length of this address forbids.

The second public laboratory was opened in the same year (1683) at Stockholm, under the patronage of Karl XI of Sweden, and under the guidance of Urban Hiärne. Both institutions issued publications bearing the general title Acta[62] which may be regarded as forerunners of the "Contributions from the Laboratory of ——— University" now so common in periodical literature.

Lexicons and dictionaries have been a feature of chemical literature from the earliest times; we have already alluded to the vocabularies of the sacred art found among the Greek MSS. of the Paris National Library and referred to the tenth and eleventh centuries. These, however, seem to have been compiled for the purpose of misleading the novice in alchemical mysteries, and as Hoefer justly remarks themselves need commentaries to become intelligible.

The universal employment of symbolic characters, to represent both chemical substances and operations, rendered alchemical literature difficult of perusal and early led to the publication of keys and vocabularies explaining them. Such keys occur in the manuscripts just mentioned, in Eschenreuter's "Universal Panacea for Man and Metals"[63] which purports to have been written in or before the fourteenth century, and in the works of Oswald Crollius[64], Kircher[65], Le Febvre, Lemery and many authors of the seventeenth century; these however are not properly dictionaries.

Dr. Martin Ruland, court physician to Rudolph II, published a "Lexicon alchemiae" in 1612, and William Johnson[66] a "Lexicon [illegible]" in 1657, but the first really satisfactory treatment [illegible] in monographs alphabetically arranged was by Macquer[67] whose "Dictionnaire de Chymie" published in 1766, had great success, passing through several editions and being translated into the German, English, Italian, Spanish, Danish and Russian languages.[68] Macquer found many followers among whom

may be mentioned Nicholson (1795), Cadet (1803), and well known authors whose works are indispensable to all students of the science.

Prof. Wm. Ripley Nichols, of Boston, late chairman of this Section, in a recent private communication, has expressed the need of yet another dictionary, one of chemical synonymes. He calls attention to the large number of names by which certain chemical substances have been known at different epochs and in different professions, and to the utility of a dictionary explaining their synonymy. Professor Nichols suggests "restricting the list to terms found in the literature of the preceding hundred years, but including such alchemical names as have peculiar historic interest and such as have persisted more or less in commerce and pharmacy." He also suggests that the work should include English, French and German names, except when they are simply literal renderings of the same term, and names of those minerals which are of simple composition like halite, witherite, fluorite, etc., under their appropriate heading. In an example of the proposed method of treatment he gives a long list of names for ferric oxide under the several heads: alchemical, chemical, mineralogical, pharmaceutical, commercial and popular.

The idea is certainly excellent and we have Professor Nichols' permission to mention it publicly in the hope that some one may be incited to compile such a work. We are not aware of the existence of any modern chemical synonymicon, not having seen Berta's Dictionary of chemical terms[69], published at Padua in 1842; perhaps the nearest approach to the ideal work of Professor Nichols is Sommerhoff's Lexicon pharmaceutico-chymicum[70], published in 1701, a folio of 400 pages.

About the middle of the seventeenth century the dissemination of the views of Lord Bacon, as expressed in his *Novum Organon*, gave a great impulse to scientific investigations, and the "splendid fiction of the new Atlantis" was practically realized in the foundation of the "Royal Society for improving Natural Knowledge." The learned men, who in 1645 met in London, and there disturbed by unhappy dissensions of civil war withdrew to Oxford to report and discuss philosophical experiments, laid the foundations of an edifice destined to rise higher, endure longer and to shelter a nobler offspring than the most sanguine could have foreseen. The organization was granted a charter by King Charles II in 1662 and

three years later began the publication of the "Philosophical Transactions, giving some accompt of the present undertakings, studies and labours of the ingenious in many considerable parts of the world." In the following year the French Academy of Sciences began their Mémoires. Thus arose the element of periodicity in scientific literature, one which rapidly increased in importance.

The year 1665 also witnessed the birth of the parent of that large class of periodicals not issued by societies but published by private parties either individually or coöperatively. The first number of the "Journal des Scavans" appeared January 5, 1665, and for one hundred and thirty years was the most prominent literary and scientific journal of Europe. At that early date the periodicals partook of a literary nature rather than of the scientific, but gradually these two elements became distinct and the lines became more and more closely drawn until every branch of pure and applied science supported a serial especially devoted to its progress. Among the earliest periodicals devoted to chemistry and its associate physics may be mentioned the "Journal de Physique et de chimie," edited by the Abbé Rozier, de la Métherie and others, begun in 1770 and continued through ninety-five volumes to 1822; the "Chemisches Journal" of Lorenz Crell, 1778–86, which was followed by the "Chemische Annalen" by the same editor, extending until 1803. The "Annales de Chimie et de Physique," begun in 1789, enjoys the honor of being the oldest surviving serial of this class.[71]

Of the foundation of Societies exclusively devoted to chemistry in France, England, Germany and America, and of their periodical publications, we can make mere mention. The existence of a "Columbian Chemical Society" in Philadelphia as early as 1811, deserves passing notice. Two volumes of its Memoirs were published containing articles by Drs. Mitchell, Cutbush, Manners, Bache and others.

Of chemical bibliography and works relating exclusively to the history of the science, we have elsewhere[72] given a critical resumé.

As we proceed with this review of chemical literature, the publications increase enormously in number, variety and importance, and the end of our undertaking seems to move forward faster than we progress. It is manifestly impossible in the brief time allotted to this address to analyze the chemical literature of the preceding hundred years. How shall we attempt it portray the

theories and experiments recorded in the writings of Bergmann[73], Scheele[74] and Gahn, the eminent Swedes; of Black[75], Cavendish[76] and Priestley[77], the English triumvirate; of the French philosophers Fourcroy[78], de Morveau[79], and Lavoisier[80]; of the industrious German chemists Kunckel[81], Marggraf[82], Wenzel[83], Klaproth[84] and Richter[85]; or of the Italian philosophers Galvani[86], Volta[87], and Brugnatelli?[88] And passing to a later period, how shall we do justice to the labors of Berthollet[89], Gay Lussac[90], Thénard[91] and Laurent[92]; of the immortal Dalton[93], of the brilliant Davy[94] of the indefatigable analyst Berzelius[95]; of Mitscherlich, Liebig, Wöhler, and Hofmann; of Dumas, Berthelot and Adolph Wurtz; of Graham, Frankland, Abel and Crookes?

American contributions to chemical literature have been exhaustively discussed in the hearing of many present and need not now detain us; I refer to Prof. Silliman's Address at the celebration of the centenary of Priestley's discovery of Oxygen, held at Northumberland in 1874.[96]

The modern period of chemical literature is characterized by two opposing forces, a tendency to dispersal and an effort to collect the widely scattered publications. The multiplication of learned societies especially in Europe, each of which supports its own organ, and the increasing number of nations interested in scientific pursuits, in each of which arises an independent periodical, tend to the wide dispersal of memoirs, essays and notices.

On the other hand, industrious editors laboriously collect and set in order these scattered observations, constructing compact, massive handbooks and many volumed cyclopedias. The monumental work of Gmelin, which is even now passing through another edition, has worthy rivals in the magnificent compilations edited by Henry Watts and by Adolph Wurtz, and in the work of Beilstein in a field of special difficulty. For systematic treatises we can point to Miller, Graham-Otto, Pelouze et Frémy, Roscoe and Schorlemmer and the ever youthful Fownes.

Invaluable, too, are the annuals known as Jahresberichte, monuments of conscientious editorship.

Another feature of modern growth is the multiplication of special treatises dealing with single topics in every branch of chemistry, inorganic, organic, theoretical, analytical, technological, pharmaceutical and physiological, until mere acquaintance with their titles

becomes a serious undertaking for busy workers in the laboratory.

The amount of time and labor required to search for a given point throughout the maze of modern chemical journals, transactions, treatises and hand books, is well appreciated by my audience; the superb dictionaries and annuals referred to accomplish much in cataloguing, condensing and systematizing the infinity of observations; nevertheless, all working chemists feel the need of special indexes, simply arranged, as complete as possible and accessible to every one. Sharing in this feeling, I have contributed my mite to the literature of chemical indexes and have inspired several friends, fellow-members of this Association, with the courage to follow. Uranium[97], Manganese[98], Titanium[99], Vanadium[100], Ozone[101], and Peroxide of Hydrogen[102] have been indexed on a uniform plan, and my pupil W. W. Webb, B. S., has prepared an Index to the Literature of Electrolysis[103], which will soon be published in the same channel as the others;— the Annals of the N. Y. Academy of Sciences.

Pardon, we beg you, these personal allusions: we have indulged in them with the object of bringing before this Section a proposition for organized effort in the preparation of Indexes to all the Elementary Substances on a uniform plan.

The advantages which would accrue from the publication of special indexes to the literature of the elements and of such other subjects as may prove desirable are too obvious to need argument; I have thought that the undertaking might be placed in the hands of a Committee of the Chemical Section of this Association. The existence of an Index Society in England under the guidance of Henry B. Wheatley, F. S. A., Hon. Sec.,[104] affords a precedent, if indeed any be required.

The first duty of the Index-Committee of the Section of Chemistry would be to secure volunteers willing to undertake the compilation of special indexes; surely in this National Association a few chemists can be found ready to coöperate in such an important work. The Committee would further adopt a uniform plan for the indexes, determine the method or channel of their publication, and exercise general editorial supervision over the undertaking.

Hoping that this suggestion will receive your approbation I leave it in your hands.

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* *Works marked with an asterisk are in the private library of the author.*

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PAPERS READ.

On a New Microchemical Method of Determining the Feldspars in Rocks. By J. Szabó, of Budapest, Hungary.

My method of determining the feldspars in rocks is based chiefly on the coloration of the flame by potash and sodium, and on the degree of fusibility of the different kinds of feldspars.

In order to obtain the same results, the circumstances under which the experiments are made must be the same. The coloration of the flame and the fusibility are observed at the same time, in the same experiment.

I take a Bunsen burner of the dimensions described by Bunsen, the lamps being those furnished by Desaga in Heidelberg. I use two parts of the flame, one without the chimney at a height of five millimetres above the lamp for a lower temperature, and one for the highest temperature, which is produced by putting the chimney on the lamp and using a height of 5–10 millimetres above the chimney. It is the "Schmelzraum" of Bunsen, the region of fusion, and he estimates its temperature at 2300° C.

The platinum wire for introducing the feldspar grain must be very thin, a piece of one decimetre length should not weigh more than thirty-two milligrammes. I take pieces of it of three centimetres in length, connect them by fusion with a glass tube at one end, and at the other end, a ring is made of about one millimetre diameter, on which the grain of feldspar is fixed by means of distilled water.

The size of the feldspar must be approximately the same; it is the size of a fine mustard seed. If perfectly fused, it should be about this size (•). The feldspar grain is introduced into the different parts of the flame by fixing the glass tube to the holders used in the observations for spectrum analysis.

New determination of the fusibility of the minerals.

The result of determining the fusibility of the minerals being in many respects different from the results as obtained by Von Kobell, I find it necessary to give a brief description of my method of obtaining the degrees of fusibility.

For determining the fusibility with the same platinum wire and of the size mentioned, I use three parts of the Bunsen flame: *a*, the base of the flame; *b*, the height of five millimetres as mentioned, and *c*, the region of fusion, "Schmelzraum." Now I give the degrees of fusion according to the results obtained in those three experiments, and discriminate the following *eight* degrees for the fusibility of the minerals in general.

0. Infusible in all the three cases. Quartz, spinel.

1 (I = 0; II = 1) In five millimetres height during one minute no change is visible; but in the highest temperature after being exposed for one minute, the points and edges begin to be a little rounded, but on the sides no change is remarked. Bronzite, hematite, some anorthite, and some augite (Monté Rossi, Etna).

2 (I = 1; II = 2). The base of the flame serves in most cases only to fix the grain to the platinum wire. I speak of the first experiment, when the grain is introduced into the flame at five mm. height, and I call the second experiment obtained in the highest temperature. The mineral, to which I give the second degree of fusibility, shows after having been exposed for one minute to the flame in five mm. height a change in the points and edges, but the faces keep their original condition. In the second experiment even the faces began to fuse, but without being rounded. Some diallogite, bytownite.

3. (I = 1–2; II = 3). In the first experiment (five mm.; one minute) the points, edges and faces show the fusion but the general form of the grain is not changed. In the second experiment (region of fusion; one minute) the whole grain begins to be rounded, without becoming globular. Adularia (St. Gothard), labradorite, magnetite.

4. (I = 2–3; II = 4). In the first experiment (fifty-five mm.; one minute) it fuses without forming a globule; but in the second experiment it gives a perfect globule. Some hypersthenite, amphibole, most kinds of augite.

5. (I = 4). It fuses to a globule in the first experiment (five mm.; one minute); in this case the second experiment is not re-

quired. Petalite, garnet, some albite (of the trachytes of Hungary).

6. It fuses to a globule at the height of 5 mm. in less than one minute, or in the base of the flame but after the red heat. Borax, cryolite.

7. It fuses to a globule after some seconds near the base of the flame, before becoming red hot. Stibnite, sulphur, ozokerite, etc.

The results may be sometimes intermediate, for instance: 6–7 for rock salt, which begins to be fused at the commencement of a red heat; and the degree of fusion would be indicated as 5–6, 4–5, etc.

The exclusive use of Bunsen's burner in determining the fusibility is an advantage of my method as compared with the method of Plattner and Von Kobell, because the result is not affected by subjective ability of handling the blow-pipe. The temperature is nearly always the same, the gas flame used by me is never higher than fourteen centimetres. The time is also fixed, it is one minute.

The use of a lower and higher temperature and the observation of the change with a lens are in some cases very interesting. It is sometimes found that the substance is a mixture, for example, of feldspar and quartz; the feldspar is fusible, and the points and edges of the infusible quartz become visible in the mass in the first experiment, while at the high temperature the two substances may give one fused mass.

The chemical elements or the invariable binary compounds (quartz, magnetite, cassiterite, etc.) have a constant degree of fusibility, but this is not the case with compounds in which the isomorphic elements replace each other, or which are variable in the relative quantity of their elements, and that is the case with by far the greater number of minerals and especially with the feldspars. The presence of some elements make the mineral more or less fusible. Sodium is the element in the silicates which makes them easily fusible; the magnesium and aluminum render them less fusible, or altogether infusible. This important fact has induced me to establish the standard for the degree of fusibility according to the circumstances under which the fusion takes place, and not to take a mineral representing the degree, because such minerals are not to be found for all degrees among the elements, or the double constant compounds and the more complicated minerals, as amphibole, orthoclase, garnet, etc., may be variable, even when obtained from the same locality.

The fusibility of the feldspars, amphiboles, augites, as the chief rock-forming minerals, varies between 0–5.

The feldspars.

According to the dominant protoxide element there are potash, soda and lime-feldspars, which are passing into each other not only according to the silicic acid but also according to the protoxide element.

Tschermak discerns in the three kinds of feldspars on the "principle of equidistance" of the characteristic protoxide the following ten series.

A. Potash feldspar (orthoclase).

1. Adularia	series	K_2O	16–13 per cent
2. Amazonite	"	"	13–10 " "
3. Perthite	"	"	10–7 " "
4. Loxoclase	"	"	7–4 " "

B. Sodium feldspar (sodium plagioclase).

5. Albite	series	Na_2O	12–10 per cent
6. Oligoclase	"	"	10–8 " "

C. Lime feldspar (calcium plagioclase).

7. Andesine	series	Ca O	6–10 per cent
8. Labradorite	"	"	10–13 " "
9. Bytownite	"	"	13–17 " "
10. Anorthite	"	"	17–20 " "

If the dominating protoxide elements show this change, the other elements vary in their relative quantity, of which the limits are given in the following table, taken from a considerable number of reliable analyses.

	Na_2O	K_2O	Ca O	Al_2O_3	SiO_2
1. Adularia	1–2	16–13	0–15	17–19	67–64
2. Amazonite	2–4	13–10	0–15	17–19	67–64
3. Perthite	4–6	10–7	1–15	17–19	67–65
4. Loxoclase	6–8	7–4	1–15	19–20	67–65

	Na_2O	K_2O	CaO	Al_2O_3	SiO_2
5. Albite	12–10	0–1	0–1	18–20	69–66
6. Oligoclase	10–8	1–2	2–6	21–24	65–61
7. Andesine	8–5	2–1	6–10	24–29	60–54
8. Labradorite	5–3	2–1	10–13	25–30	56–50
9. Bytownite	3–1	1–0	13–17	29–34	52–47
10. Anorthite	2–0	1–0	17–20	32–37	46–43

If we take into consideration the results of the chemical, optical and crystallographical study of feldspars, we must distinguish: (*a*) in the class of potash feldspars, chemically, the adularia, amazonite, perthite and loxoclase; optically, orthoclase and microcline; (*b*) in the class of sodium feldspars, from a chemical point of view, albite and oligoclase; from an optical point of view, soda, plagioclase and the very rare soda orthoclase; (*c*) in the class of the lime feldspars the dimorphism has not yet been observed, therefore we discern only the four chemical species: andesine, labradorite, bytownite and anorthite, which on optical and crystallographical grounds are to be considered as four distinct species.

According to Tschermak's classification ten series have been established; there are intermediate series too, and in the same manner it is possible to distinguish less than ten series. From a geological point of view it is enough to establish the following five species: orthoclase, microcline, oligoclase, labradorite and anorthite.

The potash feldspar, as a rock-forming mineral, in most cases corresponds with the amazonite and perthite series, while adularia is restricted to the crystals found in veins and cavities of some orthoclastic rocks. It is therefore, for the geologist, sufficient to make use of the general name of orthoclase for the potash feldspar, except in the case when the microscope shows the presence of the dimorphic state, then the microcline must be named too. The microcline is very common in the old orthoclastic rocks, but in the newer volcanic rocks, containing the feldspars in a glassy state, I have never observed it.

In the series of the soda feldspars albite is restricted to the minerals found in the cavities of certain rocks, while the soda feldspar as a rock forming mineral is usually the oligoclase. In geological respect the oligoclase and the andesine have nearly the same

rôle, therefore generally they might be taken for one species; the distinction may be reserved for a more detailed classification.

The labradorite is the main representative of the soda lime feldspars, but variable too. It stands sometimes nearer to the andesine; at other times nearer to the bytownite series.

The true lime feldspar is anorthite with bytownite, which are nearly always found together.

In the same rock it is very rare to find only one kind of feldspar; there are at least two, if not more, to be found together, but one of them is usually dominant, and then it is entitled to enter into the nomenclature of the rock.

The coloration of the flame.

The yellow coloration of the flame by the feldspars is a constant character; I have never found a feldspar without sodium, though analyses are met with in which the sodium has not been mentioned.

The coloration caused by sodium is very intense; it overcomes every other color, and this is also the case with the red flame produced by the presence of potash.

To observe the coloration produced by potash, I prefer to make use of the solution of indigo in concentrated sulphuric acid prepared as described by Fresenius, but diluted only so far, that the white-heated platinum wire begins to be visible through it. Cobalt glass is good only when it contains a high percentage of oxide of cobalt.

The calcium combined with silicic acid never shows any coloration.

I observe five degrees in the yellow flame of sodium:

1. The yellow flame is very small. It is obtained by introducing into the flame the spout of Monte Somma. It corresponds to 0·05 to 0·1 per cent of Na_2O in the feldspar.

2. The yellow flame is a little larger. It is shown by the potash-free varieties of sodalite from Mexico. It indicates 1–2 per cent Na_2O, as the analyses made by Brush only the potash was sought.

3. The yellow coloration is stronger. Observable in the hyalophane of the gneiss of Vesuvius. Shows 2–4 per cent Na_2O.

4. The yellow coloration is intense. Shown by the potash-free varieties of the Vesuvian sanidine, 4–8 per cent Na_2O.

5. The most intense yellow flame, oligoclase, St. Gothard, 8–16 per cent, Na_2O.

The red coloration produced by potash is very weak when the feldspar is exposed to the high temperatures by itself, and becomes visible only in such feldspars which contain more than four per cent of it. Now all soda and lime feldspars contain less than four per cent K_2O; therefore, if a red coloration is visible in the first two experiments, the feldspar is a potash feldspar of a greater or less percentage, and accordingly I discern three degrees.

1. Scarcely visible, doubtful. Loxoclase, Hammond, 4–10 per cent, K_2O.

2. Distinctly visible. Sanidin, Perlenhardt, Rhine, 10–13 per cent, K_2O.

3. Very distinctly visible. Adularia, St. Gothard; leucite; 13–20 per cent, K_2O.

In order to be quite certain as to the potash, a third experiment is made, by adding gypsum to the feldspar, by which the alkalies are converted into sulphates, and thus rendered volatile in such a manner that even a very small part of one per cent becomes very distinctly observable. As regards the sodium, the five degrees remain the same as in the first two experiments, but for the potash four degrees can be discerned, of which the first two relate to the plagioclase, the last two to the orthoclase.

1. Very weak and visible only for some seconds. Anorthite of Monte Somma, 0–1 per cent, K_2O.

2. Weak but very distinct and lasts for two minutes. Andesine from the trachyte of Déva (Transylvania), 1–4 per cent, K_2O.

3. Strong red flame, clearly visible for two minutes. Sanidin of Perlenhardt (Rhine), 4–13 per cent, K_2O.

4. The red flame is very strong and permanent for more than two minutes. Adularia of St. Gothard, or leucite of Albano, 13–22 per cent, K_2O.

Fusibility and the quality of the fused matter.

It has been mentioned that the degree of fusibility, as regards the feldspars, varies from zero to five degrees; but besides this character, there is a remarkable difference in the quality of the fused matter. In all cases, when a feldspar is fusible, it assumes this state slowly without intumescence; if this phenomenon is observable, for instance as is the case in many zeolites, the substance is not a feldspar.

The fused matter may be a pellucid or transparent glass, or an opaque enamel. The glass may contain bubbles or not; the enamel is sometimes gray, sometimes white and may contain bubbles. The bubbles are situated for the most part inside, and the surface of the globule is smooth, but many of the potash feldspars exhibit the peculiar property of having also outside bubbles; this is the case especially with the orthoclases of granites and other old rocks, while the glassy potash feldspars never show this character. Whenever the microscope gives us notice of the presence of microcline, the outside bubbles are never wanting.

Practical course of the determination.

The Bunsen burner, and two holders on each side of it to the right and left, are placed on a thick piece of glass, in order to secure a smooth motion. Two feldspars may be determined at once, or one feldspar, and on the other holder may be put a typical feldspar for comparison. If the grain will not adhere to the moistened ring of the platinum wire, the ring may be too large or too small for the size of the mineral fragment. Sometimes the platinum takes up no water; in that case I place the grain on the ring by means of a finely pointed pencil, or any other non-elastic matter.

The mineral fragment must be looked at with a Coddington lens in order to observe the slightest change in the consecutive experiments.

There are three experiments to be made, the first two with the mineral alone, but introduced at different temperatures for one minute; the third is made in the high temperature, but with addition of gypsum, and exposing to this high temperature for two minutes. The results observed are inscribed in blank forms, which must have columns for every observable phenomenon, so that after the three experiments we get a group of characters, which enables us to give the name of the feldspar, supposing that it was in a normal condition, or at least not far from it.

The next table shows the order of succession of the three experiments and their numerical results, not only for the ten series of feldspars, but also for some feldspathic minerals found in volcanic rocks, especially of newer date. At the end, two common minerals, the amphibole and augite of one special locality in Hungary, are exhibited in their somewhat unusual behavior; both those minerals being usually fusible 4–5.

TABLE

SHOWING THE ORDER OF SUCCESSION OF THE FLAME-EXPERIMENTS AND THEIR NUMERICAL RESULTS FOR THE FELDSPARS, AND SOME ASSOCIATED MINERALS.

FELDSPARS.	I Experiment. 5 mm. 1 minute.				II Exper. Region of fusion. 1 min.				III Exp. with gypsum.		Flame reaction of the HCl solution.
	Na 1-5	K 1-3	Degree of fusion, 1-5	Quality of the fusion.	Na 1-5	K 1-3	Degree of fusion, 1-5	Quality of the fusion.	Na 1-5	K 1-4	
1 Adularia	1-2	3	1-2	Glassy, sometimes bubbles outside.	2-3	3	3	Often bubbles outside.	2-3	4	Ca 0, Na 1-2, K 0.
2 Amazonite	2-3	3-2	2-3	"	2-3	3-2	3-4	"	3	3-4	Ca 0, Na 1-2, K 0.
3 Perthite	3-4	2	3	"	3-4	2	4	"	3-4	3	Ca 0, Na 1-2, K 0.
4 Loxoclase	4	1	3-4	"	4	1	4-5	"	4	2-3	Ca 0, Na 1-2, K 0.
5 Albite	5	0	3-4	Glassy, contain bubbles.	5	0	4-5	Bubbles inside.	5	0-1	Ca 0, Na 1-2, K 0.
6 Oligoclase	5-4	0	3-4	Glassy, or enamel-like.	5-4	0	4-5	"	5-4	1-2	Ca 0, Na 2, K 0.
7 Andesine	4-3	0	2-4	Glassy, transparent.	4-3	0	3-5	"	4-3	1-2	Ca 0-1, Na 2-3, K 0-2.
8 Labradorite	3-2	0	1-2	" "	3-2	0	2-3	Enamel-like.	3	1-2	Ca 1-2, Na 2-3, K 0-1.
9 Bytownite	3-2	0	1	Glassy, whitish.	3-2	0	1-2	Glassy, with some bub's.	3-2	0-1	Ca 2-3, Na 1-3, K 0-1.
10 Anorthite	2-1	0	0	Nearly no change.	2-1	0	0-1	Glassy, without bubbles.	3-2	0-1	Ca 2-4, Na 1-3, K 0-1.
Leucite	2-3	3	1	Glassy, translucent.	2-3	3	2	Glassy, no bubbles.	2-3	4	Ca 0, Na 2-3, K 4.
Nephelite	5	1-3	2-3	Glassy, enamel-like.	5	2-3	3-4	Glassy, with rare bubbles outside.	5	3-4	Ca 0, Na 5, K 3-4.
Nosite	5	1	1	A little glassy, grayish.	5	1-2	2	Partly translucent.	5	2-3	Ca 0, Na 5, K 3.
Hallynite	5	1-2	1	Nearly no change.	5	1-3	2-3	Partly glassy.	5	2-3	Ca 0, Na 5, K 1-3.
Sodalite	5	0-3	1-3	Glassy, whitish.	5	0-3	3-4	Glassy, bubbles.	4-5	0-3	Ca 0-1, Na 5, K 1-3.
Amphibole	2	0	1-2	Black, no lustre.	2	0	4	Black, shining.	3	1	Ca 0, Na 1-2, K 0.
Trachyte, Drégely var Augite	1	0	0	No change.	1-2	0	1-2	Black.	2	0-1	Ca 0, Na 1, K 0.
Trachyte, Drégely var											

In the *first* experiment, without the chimney, the mineral is introduced at the lower temperature (5 mm.) for one minute. The coloration caused by the sodium is estimated, and then immediately the observation with the blue indigo-fluid is taken in order that it may be completed at the end of one minute. After this time the holders are removed and the grains observed with a lens in order to give a number to the degree of fusibility and to its quality. All results are noted in their respective columns on the form.

Second experiment. The chimney is put on, the platinum wire is placed at such a height, that it may reach the region where the highest temperature is obtainable. All the observations are the same as in the first experiment, except that for the degree of fusibility as well as for its quality which is now conclusive.

Third Experiment. The same grain is exposed to the same temperature again, but after having been moistened with distilled water, and brought in contact with powdered gypsum, in order to cover it with it on all sides, and in that state it is introduced into the flame for two minutes. Now only the degrees for sodium and potash are noted.

General remarks for the three kinds of feldspars. For the *potash feldspars* the presence of this protoxide element in a higher percentage is the most essential character, and this is determined by the first two experiments, the potash feldspars showing alone the red flame and none of the plagioclastic, soda, or lime feldspars. The first experiment will therefore determine whether the feldspar is an orthoclase or a plagioclase.

The potash feldspars are not acted upon by concentrated hydrochloric acid; there is only a very small quantity of sodium extracted, so that the solution introduced into the flame produces a feeble yellow tint.

For the *soda feldspars* the great quantity of sodium is characteristic, and visible at the very moment when the mineral is introduced into the flame. At the same time the red flame must be absent. In the third experiment even the small quantity of potash contained in the soda feldspars becomes visible. The oligoclases contain always more or less potash, but among the albites there also occur specimens without potash.

The oligoclase is very commonly found together with the orthoclase in the same rock, and I have observed that whenever

the orthoclase is poor in potash (loxoclase) the oligoclase contains also very little of it; but with the amazonite or perthite a variety of oligoclase is always associated, in which the percentage of potash is correspondingly higher.

The soda feldspars are not acted upon by concentrated hydrochloric acid; the solution gives only a slight yellow coloration to the flame.

The *lime feldspars* are characterized by the gradual intensity of the yellow flame, the absence of the red flame, and by the lower degree of their fusibility. The degree of fusibility was not an essential character of the potash and soda feldspars, but it becomes very important in the case of the lime feldspars, the four series being, according to their increasing basicity, less and less possible.

The degree of	fusibility	for	the	andesine		is	5–3
"	"	"	"	"	" labradorite	"	3–2
"	"	"	"	"	" bytownite	"	2–1
"	"	"	"	"	" anorthite	"	1–0

The quality of the fused material shows also some slight peculiarities. The anorthite is not fusible, or fusible only at the points, the fused part is glassy, and contains no bubbles; the bytownite is similar in behavior but with a somewhat more advanced degree of fusibility; the fused labradorite is in most cases a whitish or grayish enamel, not unlike to some chalcedony; the andesine shows this character only in the first experiment, but in the second it gives a transparent or a limpid globule with bubbles inside, like the oligoclase.

It must be remembered, that while for the highest and lowest degrees of fusibility the size of the grain has no essential influence, if we take a fragment of quartz only the tenth part of the size required by the method, it will show the infusibility in the same manner, and on the contrary, if we take of borax ten times as much, it will melt nearly as easily as the standard size; but it is quite different with the middle and neighboring degrees. If the grain of labradorite is somewhat smaller, it shows the fusibility of andesine; in that case the experiment must be repeated with a somewhat larger fragment: if it fuses, it is andesine, if not wholly fusible, it is labradorite.

The concentrated hydrochloric acid exerts very little action when left in contact with the andesine, but the labradorite is partly decomposed, the bytownite still more, and much more the anorthite, the protoxide elements being accordingly converted into chlorides, which are very effective in coloring the flame.

The flame experiments with the hydrochloric solution.

The application of this well known principle is carried on in the following manner: I put in a short glass tube (height about 2 cm., diameter 5–6 mm.) some fine fragments of the mineral, and add only as much of the concentrated hydrochloric acid, as is required to cover it completely. The glass tube covered with wax is left for twenty-four hours; on the next day, at the same hour, I take out a drop by plunging in a thick platinum wire (like horse hair), which ends in double spiral rings. While the thin platinum wire, which we have employed for the flame reactions with the solid matter, would produce an instantaneous phenomenon, of which, I dare say, no use could be made, the platinum wire of the thickness mentioned will separate the chlorides according to their volatility. I introduce the drop into the base of the flame on the front side, and the following phenomena are to be observed. The first is the yellowish red coloration caused by the calcium chloride, but mixed with the yellow of sodium. To be certain that lime is present even in small quantities, I use a direct-vision spectroscope which shows the red and orange line very distinctly even for a trace of calcium. At the next moment the great bulk of the yellow coloration makes its appearance, and when arrived at its culmination, the red flame of the potash chloride may be observed through the indigo solution. The last of the usual colorations is that caused by lithium, visible through the spectroscope.

The degrees are marked about in the same manner as for the experiments in the dry way (see the table).

The platinum wire must of course be tried first alone for the coloration of the flame, and then moistened with the same acid, which is used for the decomposition of the mineral powder.

On the accompanying table we find the leucite as another potash mineral, having similar characters as the adularia in the dry experiments; but if we consider the flame reaction of the hydro-

chloric solution, then the great difference between the two will be evident. The same is the case with nepheline, nosite, sodalite or soda minerals; we find in the columns of the hydrochloric solution the greatest quantity of sodium, together with a high degree of potash, while the soda feldspars color with their hydrochloric solution the flame for soda very feebly, and for potash not at all.

For finding the calcium in such compounds, which are not acted upon by hydrochloric acid, for instance to make a distinction between oligoclase and andesine, I make use of the microchemical method of Boricky, by attacking the feldspar by hydrosilicofluoric acid, which shows the peculiar forms of calcium-silico fluoride in the andesine in larger quantity, and very little or none in the evaporated drop of the oligoclase solution.

Concluding remarks.

The method of determining the feldspars can be employed also for other classes of minerals with equally good results, but of course the alkalies giving the coloration, for minerals not containing alkalies, the result will be only the determination of fusibility. For the zeolites it gives some groups not only according to their volatile constituents coloring the flame, but also for the quality of the fused material, some being fusible with intumescence, some without it. It is very important also for the great class of the bisilicates (anphibole, augite, diallage, hypersthenite, enstatite, etc.), and it shows that some of them contain in more cases potash or sodium, or both of them, than has hitherto been supposed. There is a peculiar class of silicates however, the micaceous minerals, for which the result in respect of the quantitative indications is not the same as for the feldspars. The coloration caused by the potash never corresponds with its percentage, it is always less. For this class of minerals I make use of the method of Boricky.

The results obtained for the feldspars will give preliminary indications for their chemical analysis, knowing approximately the limits for the sodium and potassium; the limits for the other elements are given too. But its great importance in the lithological study of the crystalline rocks may be inferred by the fact, that it

is the unanimous conviction with petrographers, that every kind of a rock mass can be best determined by the mineral association found in it, and of all minerals it is the kind of feldspar which is the most important. There was a time, when we have been contented to say feldspar generally; then came a time when we said orthoclase and plagioclase, but now we know that this is not enough; we must discern among the plagioclases according to their basicity at least an oligoclase-andesine, a labradorite and a bytownite-anorthite. When we take a certain cycle of a well defined eruption, for instance, the eruption of the trachytic family, we find a correlative between the feldspar and the relative age: the trachyte with orthoclase-oligoclase, having the most acid constitution, was the first product of eruption, and the trachyte containing bytownite-anorthite and correspondingly having the most basic constitution, was the product of the last eruption, while the labradorite-trachytes are to be found between these two extremes. And that is the case for the cycles of other eruptions too, as far as some instances indicate it.

In the normal state of a rock it is the whole mineral association which must be taken into consideration, but in many cases the dark colored minerals are entirely changed, and only the feldspar to be found chemically in its normal state; then the determination of the feldspar is the only possible way of saying something of the lithological and chronological character of the rock in question, and the flame experiments furnish us with the result required in a short time and with sufficient exactness for the purpose aimed at.[1]

[1] Eine neue Methode die Feldspathe auch in Gesteinen zu bestimmen, von Dr. J. Szabó. Budapest, 1876. Berlin, Friedländer.

Elemente einer neuen chemisch mikroskopischen Mineral und Gesteinsanalyse, von Dr. E. Boricky. Prag, 1877.

Application of Organic Acids to the Examination of Minerals. *Third Paper.* By H. Carrington Bolton, of Hartford, Conn.

37. In the two papers previously published under the same title[1] we have shown that citric acid has a power of decomposing minerals little less than that possessed by hydrochloric acid, and that the organic acid can be advantageously employed in field-work for assistance in determining species. In the second of these papers we stated that the results obtained by the *prolonged* action of citric acid on minerals differed greatly from those recorded in the accompanying tables, and gave notice of our intention to return to the subject at a subsequent period. We have continued our researches and gained a few additional facts which we deem of sufficient interest to warrant a third or supplementary paper on the subject.

The results given in the tables (here reproduced for convenience) were obtained by acting on the minerals in fine powder: first, in the cold for a few hours, and secondly, at a boiling heat for a few minutes. We find, however, that many species which resist the brief action of a boiling concentrated solution of citric acid are more or less completely decomposed by prolonged contact with the same solution at the ordinary temperature of the work-room, say 60° to 70° Fahr. But a small number of the two hundred minerals embraced in the tables were submitted to the prolonged action of the acid solution, and those tested were selected as representative of groups, sulphides, oxides, silicates, etc. The progress of decomposition was noted at various periods from a few days to more than two years. A solution of citric acid concentrated in the cold was used in all the experiments.

38. *Sulphides.* Chalcocite showed signs of decomposition at the end of ten days, and at the expiration of several months yielded a partial solution of a pure green color.

Ullmannite showed signs of decomposing in two days, and afforded in a month a very dark colored solution.

Arsenopyrite was attacked in a few days. Pyrite showed decided evidence of decomposition in eight days; one month later the solution acquired a reddish yellow color and reacted for iron and sulphuric acid.

[1] Annals N. Y. Acad. Sciences. Vol. I, p. 1, 1877, and Vol. II, p. 1, 1880.

Chalcopyrite acts similarly, the solution being of a greenish yellow hue and reacting for copper and sulphuric acid. One gramme of the mineral lost about eleven per cent of its weight after fourteen months' contact with the acid solution. In general quantitative experiments were not conducted, for it soon became clear that the amount of decomposition was proportionate to the time and would require more attention than the results to be gained warranted.

Smaltite was decidedly attacked after eight days — the presence of sulphuric acid in the colored solution was noted. Tetrahedrite was strongly attacked in about four months. Cinnabar, orpiment, argentite and pyrargyrite resisted the acid solution altogether.

39. *Oxides.* The experiments recorded in this brief supplement were made with only those minerals which resisted the acid solution on boiling, and since it was obviously useless to test such bodies as quartz, corundum, rutile, etc., only the three oxides of iron remained of this class for examination.

Magnetite and limonite were strongly attacked in eight days; hematite yielded much more slowly, showing decided decomposition after several months.

Exposure to sunlight, as might be expected, diminished the intensity of the colored iron solutions.

40. *Silicates.* Of the silicates examined datolite is by far the most quickly decomposed by the acid solution; after twenty-four hours it yields gelatinous silica. Datolite should probably be transferred from column H of the tables to column D.

Hornblende, pyroxene, almandite, epidote, vesuvianite and serpentine were decidedly decomposed in eight days; after one month hornblende, almandite and epidote yielded colored solutions and slimy silica, and serpentine gave gelatinous silica. After fourteen months contact with the acid solution (occasionally agitated) the hornblende and pyroxene were very completely decomposed, the solutions having a dark brown color and full of floating silica. Actinolite seems to be less easily decomposed than common black hornblende. The serpentine after fourteen months yielded a dry gelatinous mass.

The feldspars are very unequally attacked under similar conditions. Labradorite yields the easiest, decomposition being noticed after eight days and slimy silica forming in the course of some months. Orthoclase and oligoclase showed marked signs of decomposition after eight months, but albite is doubtful. Tourmaline

and staurolite yielded to the acid after four or five months, the latter forming a solution colored by iron and slimy silica. Talc and kyanite appear to resist the action of citric acid.

Muscovite and biotite yield very slowly, the latter showing signs of decomposition the sooner. After two years' subjection to the acid solution very minute scales of mica separate, and some slimy silica appears in the slightly colored liquid.

41. We have detailed these simple observations somewhat minutely because we believe they have an important bearing on geological disintegration and erosion. At the Saratoga meeting of the A. A. A. S., my friend Dr. Alexis A. Julien presented an exhaustive summary and analysis of all known observations, concerning the geological action of the humus acids[2]; in this paper he notes the negative character of the results I had obtained with a number of minerals, *e. g.*, pyrite, augite, hornblende, the feldspars, micas, etc., and says: "but the nature and strength of the solvent employed, and the brief duration of each trial seem at present insufficient to justify generalizations, notwithstanding the suggestiveness of certain results." My subsequent observations have confirmed his views. Generalization is still of doubtful alue, yet we are tempted to make the venture. In the following table we have arranged the chief rock-forming minerals, with a

TABLE

SHOWING APPROXIMATE RELATIVE DISINTEGRATION OF ROCK-FORMING (AND ASSOCIATED) MINERALS BY CITRIC ACID IN SOLUTION.

Quickly decomposed.	*Slowly decomposed.*	*Very slowly decomposed.*	*Not decomposed.*
Carbonates, Phosphates, Prochlorite, Chrysolite, Nephelite.	Serpentine, Pyroxene, Hornblende, Labradorite, Garnet, Epidote, Vesuvianite, Pyrite, Limonite, Magnetite, Gypsum (?).	Orthoclase, Oligoclase, Albite (?), Biotite, Muscovite, Tourmaline, Staurolite, Hematite.	Quartz, Corundum, Spinel, Beryl, Fluorite, Barite, Talc (?), Kyanite (?).

few of their associates, under four heads indicating their relative disintegration by citric acid; the position of each mineral is of course only approximate, and it is not intended to indicate that all the species in one column are equal in degree of their decomposition. The application of this table to the consideration of

[2] Proceedings Am. Assoc. Adv. Sci., Saratoga Meeting, Vol. 28, p. 311, 1879.

geological phenomena is a subject which I do not venture to discuss; if we have a right to assume that the disintegrating effect of the acids of humus differs from that of the other organic acids in degree rather than in kind, the Table may prove a small contribution to chemical geology.

TABLES

SHOWING THE BEHAVIOR OF CERTAIN MINERALS WITH CITRIC ACID, ALONE AND WITH REAGENTS.

I.

DECOMPOSED (IN FINE POWDER) BY A SATURATED SOLUTION OF CITRIC ACID.

1. IN THE COLD.

A. *Without evolution of gas.*	B. *With liberation of CO_2.*	C. *With liberation of H_2S.*	D. *With separation of $Si\,O_2$.*
Clausthalite, Leucopyrite, Atacamite, Brucite, Gummite, Pyromorphite,* Mimetite, Triphylite, Triplite, Vivianite,! Libethenite,! Olivenite,! Pseudomalachite, Wavellite, Pharmacosiderite,! Torbernite, Autunite, Ulexite,! Cryptomorphite,! Anglesite. Brochantite.!	Calcite,! Dolomite,* Gurhofite,! Ankerite,* Rhodochrosite,* Smithsonite,* Aragonite,! Witherite,! Strontianite,! Barytocalcite,! Cerussite,! Malachite,! Azurite.*	Stibnite, Galenite, Alabandite,! Sphalerite, Pyrrhotite.	Wollastonite, Rhodonite,! Chrysolite, Willemite,!‡ Nephelite, Lapis lazuli, Chondrodite, Pectolite,!‡ Laumontite,!‡ Chrysocolla,! Calamine:!‡ Apophyllite, Thomsonite,! Natrolite,!‡ Mesolite,! Analcite, Chabazite, Herschelite,‡ Stilbite, Dewevlite, Prochlorite.

2. ON BOILING.

E. *Without evolution of gas.*	F. *With liberation of CO_2.*	G. *With liberation of H_2S.*	H. *With separation of SiO_2.*
Cuprite,! Zincite, Melaconite, Goethite,* Limonite,* Allanite. (?) Apatite,* Wolframite,* Wulfenite, Crocoite, Gypsum.*	Hausmannite,† Pyrolusite,!† Manganite,† Psilomelane,!† Wad.!† Magnesite,! Siderite.!	Bornite, Jamesonite,* Bournonite.* Boulangerite, Kermesite.	Tephroite,‡ Ilvaite, Phlogopite,* Datolite,!‡ Prehnite,* Heulandite, Serpentine, Chrysotile, Retinalite, Bastite, Genthite, Gieseckite,* Jefferisite. Masonite.*
and those in A.	*and those in B.*	*and those in C.*	*and those in D.*

II.

DECOMPOSED BY A BOILING SOLUTION OF CITRIC ACID MIXED

I. WITH SODIUM NITRATE.		K. WITH POTASSIUM IODIDE.	
Silver, Mercury, Copper, Arsenic, Antimony, Bismuth, Sulphur,* Bismuthinite, Domeykite, ! Argentite, Hessite, Chalcocite, ! Tiemannite, ! Millerite, ! Niccolite, ! Pyrite, ! Chalcopyrite, ! Linnaeite, Smaltite, !	Cobaltite, ! Ullmannite, ! Marcasite, ! Arsenopyrite, ! Nagyagite, Covellite, ! Berthierite, ! Pyrargyrite, Tetrahedrite, ! Tennantite, ! Stephanite, Polybasite, ! Enargite, ! Uraninite, ! Hübnerite. *and those in C and G.*	Realgar,* Orpiment,* Cinnabar, ! Hematite,* Menaccanite,* Washingtonite,* Magnetite,* Franklinite, Braunite, Enstatite, Hypersthene, Augite, Spodumene,* Hornblende,* Actinolite,* Pargasite,*	Olivine. Almandite, Pyrope. Colophonite, Epidote. *and most of those in A, B, C, D, E, F, G, H, and I.*

III.

L. MINERALS NOT DECOMPOSED BY THE ABOVE REAGENTS.

Graphite, Molybdenite, Proustite, Fluorite, Cryolite, Corundum, Diopside, Petalite, Asbestus, Beryl, Zircon, Vesuvianite, Zoisite,	Iolite, Biotite, Muscovite, Lepidolite, Wernerite, Leucite, Andalusite, Fibrolite, Kyanite, Topaz, Titanite, Staurolite, Bowenite,	Talc, Kaolin, Ripidolite, Columbite, Samarskite, Spinel, Chromite, Chrysoberyl, Cassiterite, Rutile, Quartz, Hyalite, Anorthite,	Labradorite, Oligoclase, Albite, Orthoclase, Tourmaline, Scheelite, Barite, Celestite, Anhydrite. (Two hundred species.)

N. B.—The gases evolved are examined with acetate of lead test paper, the solutions with appropriate reagents.

The kind and degree of action are indicated in the above Tables by the following signs:—

! Completely decomposed or dissolved.
* Feebly attacked.
† The CO_2 evolved is derived from the Citric Acid.
‡ Gelatinizes.

Carbon Dioxide as a Constituent of the Atmosphere.

By Ernest H. Cook, of Newton Villa, Southville, Bristol, England.

[abstract].

Although occurring in such a relatively small amount in the atmosphere, this substance has taken a leading part in effecting geological changes and in building up strata of immense thickness. We are apt to underrate its influence. When it is remembered that were it not for its presence in the atmosphere, no coal, very little limestone, probably no vegetable, and hence little animal life could exist, we at once see its importance.

In the present communication the author calculates the absolute amount of dioxide existing in the atmosphere, notices the sources whence the supply is derived, the amount yielded by each, and the causes at work abstracting dioxide from the air. On many of these points our knowledge is so unsatisfactory and fragmentary that the author hopes the attention of scientific men may be called to a subject of such importance to the geologist, meteorologist and chemist.

Amount of Carbon Dioxide in the Atmosphere.

Two things are necessary in order to determine this, viz.: the capacity of the atmosphere and the percentage of CO_2 contained by it. Taking the lengths of the polar and equatorial diameters of the earth and of the height of the homogeneous atmosphere, it is easy to calculate the capacity of the atmosphere. This is found to be equal to 591,647,337 cubic miles, or 2,439,987,200,000 millions of kilolitres. With regard to the average amount of dioxide contained in air, experimenters differ. The older chemists Dumas and Boussingault, Loëwy and Saussure, have published results which yield a mean of 4 volumes in 10,000 of air, or 4½ parts in 10,000 by weight. On the other hand, more recent experiments conducted by Thorpe, Fittbogen and Hasselbarth, Farsky, Reiset and others, indicate that 3 volumes in 10,000 of air is the correct number.

Assuming the first of these numbers to be the true one, we find that the amount of CO_2 in the atmosphere is very nearly 1,913,686,000 millions of kilogrammes, while if we take the more

recent determinations, we find 1,435,265,000 millions of kilogrammes. These numbers differ considerably from those previously given. Thus Dumas and Boussingault say[1] that the air is equal in weight to 581,000 cubes of copper, each having a side of 1 kilometre—a number which is 40 per cent higher than the greater of the two numbers stated above. Again Roscoe and Schorlemmer[2] state that "the amount of CO_2 in the atmosphere reaches to upwards of 3,000 billions of kilogrammes" which is again 33 per cent above the same number.

Sources whence the air derives its Carbon Dioxide.

These may be grouped as follows:

(1). Combination of carbonaceous bodies.
(2). Respiration of animals.
(3). Decomposition of animal and vegetable substances.
(4). Volcanoes and other subterranean supplies. Under the first heading is included the amount produced by the burning of coal, wood, peat, etc. Taking the statistics which have recently been issued with regard to the amount of coal raised in the world, it will be found that for the last three years at least 280,000,000 of tons have been raised annually. Assuming 75 per cent of this consists of pure carbon, and allowing a further 10 per cent for the carbonaceous material thrown away with the ash, we leave 182,000,000 of tons which are annually converted into carbon dioxide. Thus there is daily added to the atmosphere from this source 1,800,000,000 of kilogrammes of dioxide. By the combustion of wood, peat, oil, etc., we may assume one-third more to be added, thus making a total of 2,400,000,000 of kilogrammes per day derived from the combustion of carbonaceous substances.

In the case of the respiration of animals we can only form an approximate estimate. The population of the world is at present about 1,500,000,000, and experiment shows that an average individual produces about one kilogramme of CO_2 in a day of twenty-four hours. Thus the human race in respiring adds daily to the air 1,500,000,000 kilogrammes. From the lower animals we may consider twice this amount to be derived; thus from the whole

[1] Ann. Ch. Phys., iii, 257, 288.
[2] Treatise on Chemistry, Vol. I, p. 449.

animal kingdom about 4,500,000,000 of kilogrammes of dioxide are daily added to the atmosphere.

The decomposition of vegetable and animal substances must obviously yield a large quantity of dioxide; but practically nothing is known of the nature of the change or of the amount produced. In order to fix our ideas we may assume that this source yields the same quantity as is yielded by man, viz.: 1,500,000,000 kilogrammes.

The last source mentioned is by far the greatest. Not only do active volcanoes produce, but extinct and volcanic districts join in increasing the quantity. Considering the immense area covered by these districts and the immense quantity of gas evolved, we must conclude that by far the greatest amount of atmospheric dioxide has been obtained from this source. In fact, Poggendorff calculated that ten times as much is produced by volcanoes as by all the other sources put together. Allowing for the amounts previously given being larger than similar numbers used by Poggendorff, we may take it that 40,000,000,000 kilogrammes are daily given to the atmosphere from these subterranean sources.

Finally, if we add together these various amounts, we find that the air is daily receiving an addition of about 50,000,000,000 of kilogrammes of carbon dioxide. Dividing the absolute amount contained in the whole atmosphere by this number, we find *that the amount of carbon dioxide in the atmosphere would be double what it is at present in about one hundred years if there were no means of compensation.* Poggendorff, who made a similar estimate, gave three hundred eighty-six years as the time required to double the amount of CO_2 in the air. The difference is explained by the absolute amount of CO_2 being at that time considered greater than it really is, and also by the amount of carbonaceous matter burnt being greater now than in his time.

The disagreeable nature of the reflections called up by this last calculation disappear, however, when we come to consider the

Compensating Influences.

The known causes at work removing the dioxide may be considered under three heads, viz.:

(1). Fixation of carbon by growing plants.

(2). Removal of dioxide by zoophytes.

(3). Absorption of dioxide by inorganic chemical actions.

The first cause here mentioned is one which is essential to vegetable growth. In estimating its magnitude we are met by the want of reliable experimental data, making it almost impossible to arrive at any definite conclusion.

Certain experiments have shown that speaking approximately, a square metre of leaf will decompose one litre of CO_2 per hour in sunlight. The area of the land capable of bearing vegetation amounts to 49,400,000 square miles. Assuming the area of leaf borne by this land to be 50 per cent of this amount, we find that about 63,726,000,000,000 of square metres of leaf are hourly engaged in purifying the atmosphere. This decomposing effect will go on for about ten hours out of every twenty-four, but will be less active in the autumn and winter months than in spring and summer.

Taking all things into consideration (allowing 25 per cent for diminution in winter) we find that vegetation daily decomposes about 478,000,000,000 of kilolitres of CO_2, or about 900,000,000,000 of kilogrammes, an amount which is much greater than the amount produced from all sources. The magnitude of the action exerted by plants is surprising, but it must be remembered that during darkness plants expire CO_2. No account is taken of this action here, as no quantitative experiments seem to have been made upon it. The author thinks that this action goes on to a greater degree than is usually supposed.

The second great action going on in nature is effected by the interposition of animal life. It consists in the removal from sea-water of the carbon dioxide held by it in solution, by certain low forms of animal life, such, for example, as the coral-builders and the foraminifera. (Certain other forms, such as brachiopoda and lamellibranchiata also fix CO_2 as calcic carbonate; their effects are, however, comparatively insignificant.) The immense influence exerted by these organisms is evident when we look at the vast masses of limestone occurring in geological formations of all ages which have an exclusively organic origin. Nor is this influence confined to the past. Recent deep-sea soundings have revealed the fact that foraminiferal life still flourishes in the depths of the ocean, while we know that coral-reefs are still being constructed in the warmer seas. In short, this cause must be considered as the chief one which is active in removing dioxide, but want of data prevents our estimating its amount.

The third action going on in nature, effecting the purification of the air, is a purely inorganic one. Included under this head are such processes as the conversion of feldspar into kaolin, the decomposition of such silicates as hornblende, pyroxene, etc. The large deposits of kaolin and decomposed feldspar, which are met with in the earth, sufficiently prove the magnitude of this action. Thus "a weight of carbonic dioxide equal to more than twenty-one times that of our present atmosphere would be absorbed in the production from orthoclase of a layer of kaolin extending over the earth's surface with a thickness of 500 metres, an amount which evidently represents but a small proportion of the results of feldspathic decay in the sedimentary strata of the globe."[3] Evidently then here we have a cause which has an important influence in effecting the removal of atmospheric carbon dioxide. It is impossible, however, to estimate its amount.

General Conclusions.

It is evident that if the compensating influences are just equal in total effect to the productive ones, the amount of carbonic dioxide in the air remains constant, but an insufficiency of data prevents a definite answer being given to this. We know, however, that these processes have been going on for a very long time. In fact, there is already fixed in the earth an immense quantity of CO_2 equal to many hundreds of times the amount contained in the present atmosphere. This must have been derived from the atmosphere, but it is not necessary to assume that it *all* formed part, at one time, of the atmosphere. It must, however, be evident that at one time more CO_2 existed in the atmosphere than at present.[4] If this be so, the compensating influences must be more powerful than the producing ones, and the proportion of atmospheric dioxide must be diminishing.

[3] Dr. T. Sterry Hunt. *Chemical and Geological Relations of the Atmosphere.* American Journal of Science, May, 1880.

[4] Unless we accept Dr. Hunt's theory that space is filled with an atmosphere similar to our own, and that CO_2 fixed in the earth at one time formed part of this universal atmosphere.

A SIMPLE LABORATORY DEVICE. By ERNEST H. COOK, of Newton Villa, Southville, Bristol, England.

[ABSTRACT.]

IN this communication the author described an improvement in water and air ovens which he has employed with advantage in his own laboratory. This consists in applying the principle of the Bunsen water pump to these instruments. Thus instead of being closed boxes as is usually employed, a current of air is drawn through them and over the substance to be dried. With this simple addition, the time of drying is greatly reduced; and without any alteration of apparatus, substances can be completely dried at a low heat, and in fact at the ordinary temperature, by simply increasing the rapidity of the current of air traversing the chamber.

The same principle is sometimes usefully applied to small portable stink cupboards which stand upon the laboratory bench. The practical details are so simple and self-evident that a description is unnecessary.

THE ACTION OF WATER AT 100° C. ON THE β PHENYLTRIBROMPROPIONIC ACID. By LEONARD P. KINNICUTT and GEORGE M. PALMER, of Cambridge, Mass.

[ABSTRACT.]

THE action of boiling water on the α phenyltribrompropionic acid has been described by one of us in the American Chemical Journal for April. Since that time the study of the β acid has engaged our attention.

This acid is obtained by the action of bromine on monobromcinnamic acid, and is a crystalline solid soluble in alcohol, ether, bisulphide of carbon and chloroform. Its melting point is 151° C. and not 48° C. as given by Glaser (Ann. 143, P. 339), nor does it

first separate out as an oil. The acid obtained by Glaser was evidently an impure substance.

On boiling with water the acid is decomposed, a bibromstyrol, whose structure we have not as yet determined, and the α monobromcinnamic acid being formed; also a small quantity of a new acid, very soluble in water, melting at 184°, which on analysis gave amounts of carbon, hydrogen, and bromine, corresponding very closely to what would be obtained if the substance was a phenylbibromlactic acid.

The formation of the same α monobromcinnamic acid, from both the α and β phenyltribrompropionic acid is not easily explained. It is probably caused by a change of position of the atoms within the molecule at the moment of its formation.

We are at present engaged on a further study of the above reaction, together with the products formed by the action of chlorine on both the α and β monobromcinnamic acids. *Chemical Laboratory, Harvard University.*

The action of Baric Hydrate on Chlortribrompropionic Acid.

By C. F. Mabery and Ralph Wilson, of Cambridge, Mass.

[ABSTRACT.]

In a previous paper by H. C. Weber and one of us, it was stated that chlortribrompropionic acid was decomposed when heated with baric hydrate. The products of this decomposition were not sufficiently studied to enable us to characterize them with any degree of accuracy, although the formation, to some extent, of tribromethylen was observed. In repeating these experiments we find that chlortribrompropionic acid, when heated with two molecules of baric hydrate in aqueous solution, is almost completely decomposed into baric bromide, carbonic dioxide and tribromethylen. The latter product was absorbed by bromine water, the ex-

cess of bromine allowed to evaporate and then crystallized from hot alcohol.

The following analysis was made:

0.1556 product gave 0.3657 grms. Ag Br + Ag Cl, which corresponds to 93.30 per cent 4 Br + Cl. Calculated for $C_2 Br_4 Cl H$ 93.40 per cent.

This substance melted at 33° and was probably identical with monochlortetrabromethan, melting point, obtained by Wallach and Bischof (Berlin Berichte, 1878–751) from the decomposition of dichloracrylic acid. Since monochloracetylen obtained in this reaction must have the constitution C H≡C Cl, the product obtained by us must have the hydrogen and chlorine atoms attached to adjacent carbon atoms. This would give for chlortribrompropionic acid the structure C Cl Br_2 C H Br COOH, and for chlorbromacrylic acid the structure C Cl Br C H COOH, which H. B. Hill has already inferred from the relations of the two disubstituted acrylic acids.

On Certain Substituted Acrylic and Propionic Acids. By C. F. Mabery, of Cambridge, Mass., and F. C. Robinson, of Brunswick, Me.

[ABSTRACT.]

By the action of bromine on the dibromacrylic acid first obtained by Fittig and Petri from dibromsuccinic acid, we have made a tetrabrompropionic acid which differs essentially in its properties from the tetrabrompropionic acid obtained by H. B. Hill, and one of us (C. F. M.), from the dibromacrylic acid derived from mucobromic.

We have studied the salts of this acid sufficiently to characterize it. When heated with baric hydrate the acid is decomposed, tribromethylen being the chief product of its decomposition. By the action of alcoholic potash hydrobromic acid is eliminated with the formation of tribromacrylic acid which melted at 117–118°.

We have also tried the action of bromine monochloride upon brompropiolic acid. Although the investigation is not yet com-

pleted we have reason to believe that the elements of bromine monochloride are taken up with the formation of dibromchloracrylic acid.

Bromine monochloride was made by the action of chlorine upon bromine at 0° in a chloroform solution.

On the Products of the Distillation of Wood at Low Temperatures. By C. F. Mabery, of Cambridge, Mass.

[ABSTRACT.]

This paper contains a description of the results which I have obtained in an examination of the volatile products of the dry distillation of wood. I have isolated acetic aldehyde and identified it by conversion into silver acetate which has been analyzed. Methyl formiate also was obtained in considerable quantity. It was saponified with plumbic hydrate and the plumbic formiate analyzed. I have found in the course of this investigation that methyl alcohol obtained by this process contains a much smaller per cent of aceton than is usually found in commercial wood spirit.

Preliminary Notice of a New Organic Base. By Albert R. Leeds, of Hoboken, N. J.

[ABSTRACT.]

When xylidine in alcoholic solution is treated with acrolein, the two bodies enter into combination to form acrolein-xylidine (its properties, analysis, etc., given), and water is eliminated. By sublimation, a limpid oil is obtained, which is cryptidine.

Description of its properties, chemical relationships, and certain of its salts.

Estimation of Dextrose in Solid Commercial Starch Sugar by loss of Rotatory Power on Solution. By Harvey W. Wiley and C. A. Crampton, of Lafayette, Ind.

[ABSTRACT.]

It is not generally recognized that the amorphous starch sugars of commerce have the same property of losing rotatory power on solution and heating as if in a crystalline state. This depends on their large content of dextrose. The principle of determination used by the authors depends on the well known fact that a freshly prepared solution of dextrose has a rotatory power twice as great as after standing a few hours or heating to the boiling point for a few minutes. The authors show by tables of analysis that the dextrose can be determined in this way with reasonable satisfaction in all starch sugars made by high conversion. Sugars, however, which still have a large percentage of maltose and dextrine will not permit of their dextrose being determined in this way. This probably arises from the fact announced by Meisl that maltose solutions increase in rotatory power in the same circumstances in which dextrose solutions decrease. The authors exhibit curves showing the rate of fall in rotatory power of dextrose solutions.

Direct Estimation of Dextrose, Dextrine and Maltose in Commercial Amylose (Starch Sugar). By Harvey W. Wiley, of Lafayette, Ind.

[ABSTRACT.]

The process is founded on a method of double reduction and polarization proposed by me at the Cincinnati meeting and since that time practically worked out.

The specific rotatory power and percentage of reducing matter of the sample are first determined. In another portion of the sample the whole of the reducing matter is destroyed by a slight

excess of mercuric cyanide and the residue, after neutralization with hydrochloric acid repolarized in a tube two and a half times longer than the first and the number thus obtained multiplied by two. This gives the specific rotatory power due to *dextrine* alone, from which the percentage of dextrine present is easily calculated from formulæ given in my paper at the Boston meeting. Having determined the dextrine, the maltose and dextrose are calculated from formulæ given in the paper.

The paper contains tables of analyses showing the results obtained by the direct method compared with those obtained by best approved methods heretofore known.

ON THE CONSTITUTION OF BENZOLE. By L. W. ANDREWS, of Brooklyn, N. Y.

[ABSTRACT.]

CLAUS maintains that Hübner's benzole formula is the one which best accords with the known facts of isomerism in the aromatic series.

This position is untenable, because the formula fails to explain the existence of three isomeric forms of bisubstitution products of benzole, unless on the basis of a method of interpretation which cannot consistently be carried out in all cases. Several considerations show that according to this formula the compound 1–2 should be identical with 1–4. These points are mutually interchangeable without rupture of the bonds or change in their distribution.

This fact is an infallible criterion of the identity or non-identity of the bonds 1–2 and 1–4. A model is shown to render apparent to the eye the peculiarities and imperfections of the "spider web" formula of *Hubner* and *Claus*.

Some Tea Analyses, with Special Reference to the Determination of Caffeine.

By W. Hodgson Ellis, of Toronto, Canada.

[ABSTRACT.]

The variation in the percentage of caffeine in tea leaves, reported by various chemists, is considerable, a circumstance due to the various methods of analysis.

After a number of trials of different methods, I have adopted the following process which gives in my hands the best results and which I have now used for several years.

1. The powdered tea is exhausted with boiling water under an inverted condenser.

2. The filtered solution is evaporated to dryness on the water bath with magnesia.

3. The residue is exhausted with absolute alcohol, and the filtered solution mixed with sand and evaporated on the water bath.

4. The residue is exhausted with hot distilled water and the solution again evaporated.

5. The residue is taken up with ether and the ethereal solution evaporated in a tared beaker and weighed.

By this method the caffeine is obtained colorless, and well crystallized.

Commercial tea as met with in Canada yields by this process from 1.5 to 3.5 per cent of caffeine.

In exhausted leaves, *i. e.*, leaves treated with boiling water, as in the ordinary method of making tea, the caffeine is nearly all removed, as in the following examples of three Japan teas and their exhausted leaves.

	1		2		3	
	Tea	Exhausted leaves.	Tea	Exhausted leaves.	Tea	Exhausted leaves.
Caffeine.....	2.5	.6	2.4	.5	3.7	.9
Ash.....	7.2	4.1	6.8	4.0	6.5	3.9
Soluble ash....	3.8	.8	3.5	.8	3.4	.7
Silica.....	.9	.8	.7	.8	.5	.7
Extract.....	37.8	16.8	40.3	13.4	33.2	8.8

In the following analyses the caffeine was determined by this method; and the extract, by boiling the powdered tea under an inverted condenser with successive portions of water. The ash, soluble ash and silica were also determined in each case.

GREEN TEAS.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	30.8	5.0	3.6	0.5	2.2	19	28.7	6.8	3.0	0.5	2.4
2	43.4	4.1	3.7	0.2	2.4	20	31.8	6.4	3.3	0.5	2.6
3	43.4	6.2	3.6	0.4	2.3	21	34.7	6.1	3.7	0.3	2.6
4	45.2	3.6	3.6	0.3	2.0	22	34.2	6.8	4.1	0.5	2.7
5	38.0	4.4	3.8	0.3	2.5	23	33.7	6.2	3.4	0.3	1.9
6	39.2	3.0	3.3	0.5	2.6	24	33.0	7.7	3.9	1.1	2.1
7	45.2	5.2	3.5	0.5	2.3	25	34.8	6.8	4.0	0.3	2.6
8	40.2	4.0	3.4	0.3	1.9	26	35.2	6.7	3.0	0.9	2.1
9	41.2	6.1	3.9	0.5	1.9	27	35.0	6.9	3.7	0.8	2.4
10	39.4	3.8	3.6	0.7	1.3	28	36.4	6.9	3.2	0.9	2.9
11	37.9	4.1	3.7	0.9	2.3	29	33.8	6.0	3.3	0.2	2.1
12	38.6	4.6	3.6	0.3	1.9	30	27.4	6.3	3.8	0.4	2.5
13	45.8	4.6	3.6	0.5	2.4	31	34.2	6.3	3.6	0.6	1.7
14	38.5	3.7	3.6	0.4	2.3	32	36.0	7.3	3.1	1.3	2.0
15	35.2	4.3	3.1	1.0	2.6	33	39.0	9.9	2.7	3.5	2.3
16	37.4	5.0	3.8	0.3	1.8	34	38.0	6.4	3.7	0.4	2.1
17	41.3	4.7	3.8	0.4	1.9	35	45.0	6.1	3.4	0.4	3.0
18	34.1	6.2	4.1	0.4	2.3						

BLACK TEAS.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	27.8	6.3	3.1	0.8	2.4	6	38.0	5.6	3.4	0.2	2.6
2	29.8	6.4	3.6	0.6	2.4	7	37.6	5.8	3.5	0.2	1.9
3	31.1	5.7	3.5	0.4	3.7	8	37.2	6.0	3.9	0.6	2.3
4	31.1	6.8	2.9	1.0	2.3	9	31.0	6.5	3.5	0.7	2.3
5	36.5	6.4	3.8	0.4	2.6	10	27.2	6.3	2.9	0.8	2.3

MIXED OR NOT DISTINGUISHED.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	29.1	5.7	3.6	.4	2.1	11	30.6	5.9	3.1	.4	3.0
2	35.6	6.9	3.8	.9	1.6	12	30.0	6.2	3.9	.1	2.3
3	26.5	5.9	3.7	.3	2.2	13	31.1	7.0	3.2	.1	2.6
4	20.2	6.8	3.0	.3	2.2	14	29.8	6.6	3.5	.4	2.2
5	26.2	5.7	3.2	.6	1.9	15	24.8	6.5	2.6	.8	1.7
6	38.2	6.2	4.0	.5	2.1	16	26.8	6.1	3.1	.5	2.1
7	35.4	6.2	3.5	.8	2.5	17	35.6	6.0	3.7	.3	2.0
8	37.2	5.6	3.7	.3	1.5	18	34.8	5.8	3.4	.2	2.6
9	23.8	6.5	3.5	.8	2.2	19	30.2	6.2	2.8	.5	2.0
10	29.2	5.4	3.0	.7	1.6	20	26.2	6.5	3.4	.2	1.8

The above analyses may be summarized as follows:—

35 GREEN TEAS.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	40.6	6.5	3.5	0.6	2.3
Maximum .	45.8	9.9	4.1	3.5	3.0
Minimum . .	28.7	5.5	2.7	0.2	1.3

10 BLACK TEAS.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	32.6	6.2	3.4	0.6	2.5
Maximum .	38.0	6.5	3.9	1.0	3.7
Minimum . .	27.2	5.6	2.9	0.2	1.9

20 TEAS MIXED OR NOT DISTINGUISHED.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	30.0	6.2	3.4	0.5	2.2
Maximum . .	38.2	7.0	4.0	0.9	3.0
Minimum . .	20.2	5.4	2.6	0.1	1.5

ANALYSIS OF THE WHOLE 65 SAMPLES.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	36.2	6.5	3.5	0.5	2.2
Maximum .	45.8	9.9	4.1	3.5	3.7
Minimum .	20.2	5.4	2.6	0.1	1.3

PEMBERTON'S METHOD FOR THE VOLUMETRIC DETERMINATION OF PHOSPHORIC ACID. By PROF. G. C. CALDWELL, of Ithaca, N. Y.

[ABSTRACT.]

MR. H. PEMBERTON has given[1] a method for the volumetric determination of phosphoric acid in fertilizers, by means of a standard solution of ammonium molybdate, which consists simply in adding this solution as long as a precipitate is produced, the final tests for further precipitation being made with small filtered portions of the solution under examination. The standard solution is prepared by dissolving 89.543 gms. of ordinary ammonium molybdate in 1000 cc. of water; each cubic centimetre of such a solution precipitates

[1] Journal Franklin Institute, March, 1882, p. 184.

3 milligrammes of phosphoric anhydride. The solution of the phosphate, first freed from organic matter and silica, is finally made up with as small an excess of acid as possible, and just neutralized with ammonia; 2 cc. of strong nitric acid and 10 gms. of ammonium nitrate are added, and the solution is heated to 60 deg., or above, preparatory to the addition of the molybdate; from the total quantity of standard solution required 0. 5 cc. is deducted for the excess necessary to neutralize the solvent action of the nitric acid.

With two modifications of the details as described by Mr. Pemberton, the process has satisfactorily sustained a careful trial in my laboratory. First, as to the preparation of the standard solution, the author found that the ordinary salt of the druggists was sufficiently pure to give the standard directly; but in order to clear away the slight turbidity which it usually leaves he added a little ammonia; it was however observed by Dr. S. B. Newbury, then analyst to the University experiment station, that in the case of the molybdate supplied to us, a notable degree of alkalinity was communicated to the solution by the necessary quantity of ammonia to clarify it; consequently the acidity produced by the 2 cc. of acid, added to the solution of the phosphate in the beginning, became so much reduced towards the end of the titration, that the results were unsatisfactory; in some instances the solution even became slightly alkaline before the precipitation was completed. Therefore, in order to secure perfect neutrality, the molybdate solution was filtered, after the addition of a few drops of ammonia at the most, and then standardized by a solution of phosphorus salt of known strength.

The second modification refers to the method of taking out the test portions to be tested for complete precipitation; even when this is done as described by the author, this method of determining phosphoric acid is unquestionably more expeditious than the usual gravimetric method with molybdate and magnesia mixture; but the necessity of filtering out each of these test portions in the ordinary manner through a filter is objectionable, and might deter many from using the method. For this filtration I used with great success a small suction filter, somewhat after Carmichael's idea, which could be plunged directly into the liquid, and through which a small portion of the same could be drawn at once into a small test tube in which the test for further precipitation was made.

The results of our trial of the method are set forth in the following table: three of the sets of determinations were made by three experienced chemists with solutions prepared by one of the number, each one being entirely ignorant of the results obtained by the others; the fourth set was obtained by another chemist with solutions of molybdate prepared and standardized by himself, and with his own solutions of the same phosphates that were examined by the others.

NO. OF PHOSPHATE.	I		II	III	IV
	Gravimetric.	Volumetric.	Volumetric.	Volumetric.	Volumetric.
237		11.18			
		11.20			
235		13.14	13.27		13.13
		13.14			13.18
					13.15
					13.19
259	12.47	12.33	12.39	12.44	12.34
	12.41	12.33		12.45	
				12.47	
				9.20	
261	9.15	9.26	9.06	9.05	9.34
	9.21	9.26		8.97	9.37
				8.97	
272	13.09	13.57	13.75	13.62	
	13.82	13.63		13.61	
		13.57		13.72	
		13.69			
		13.63			

From these results it appears that the method is capable of great accuracy, but, as in the case of No. 261, is liable to some source of error, which makes great care necessary in the preparation of the solution of the phosphate, according to the directions given by the author.

TITLES OF OTHER PAPERS READ IN SECTION C.

EXPERIMENTAL RESEARCHES ON THE SOURCES OF NITROGEN IN CROPS. By J. B. Lawes and J. W. Gilbert, of London, England.

OBSERVATIONS ON THE CONTAMINATION OF CITY WELLS. By R. B. Warder, of North Bend, O.

ON THE CAUSES WHICH RENDER FLOUR AND ORGANIC DUST EXPLOSIVE, WITH SUGGESTIONS FOR THE PREVENTION OF SUCH EXPLOSIONS. By Thomas W. Tobin, of Louisville, Ky.

SOME DERIVATIVES OF ISOPICRAMINIC ACID. By Charles W. Dabney, jr., of Raleigh, N. C.

A BENZOYL ANHYDRO ACID FROM B-METAMIDOSALICYLIC ACID. By Charles W. Dabney, jr., of Raleigh, N. C.

NOTES ON EFFECTS OF DIFFERENT SOILS UPON SOLUBLE PHOSPHATES. By Charles W. Dabney, jr., of Raleigh, N. C.

NOTE ON THE ABSORPTION SPECTRUM OF HUMIC ACID. By H. Carrington Bolton, of Hartford, Conn.

NOTES ON BONE OIL.[1] By Arthur H. Elliott and Fred. Sands, of New York, N. Y.

ON NITRO-SACCHAROSE.[2] By Arthur H. Elliot, of New York, N. Y.

TRANSMISSION OF GASES THROUGH LIQUIDS OF DIFFERENT DENSITIES. By C. G. Wheeler and F. Menzel, of Chicago, Ill.

THE SOLUTION AND LATE CRYSTALLIZATION OF GOLD HEATED WITH CHLOROHYDRIC ACID IN A SEALED TUBE. By Henry Carmichael, of Brunswick, Me.

REMARKS ON THE APPLICATION OF THE IRIDIUM KNIFE-EDGE TO ANALYTICAL BALANCES. By William Dudley, of Cincinnati, Ohio.

FIRE-DAMP INDICATORS. By Isidore Kitsee, of Cincinnati, Ohio.

NOTES ON THE ANALYSIS OF SUPERPHOSPHATES.[3] By THOMAS S. GLADDING, of New York, N. Y.

REPORT OF THE AGRICULTURAL CHEMISTS. By Chas. M. Stillwell, of New York, N. Y., Secretary.

[1] Published in full in the Journal of the American Chemical Society for Aug., 1883.

[2] Published in full in the School of Mines Quarterly for Nov., 1882.

[3] Published in full in the Am. Jour. of Chemistry, for 1883.

SECTION D.

MECHANICAL SCIENCE.

ADDRESS

BY

PROFESSOR W. P. TROWBRIDGE,

VICE PRESIDENT, SECTION D.

MEMBERS OF THE SECTION OF MECHANICAL SCIENCE:—

WHEN we look upon mechanical science as an abstract science, having for its foundation only a few simple axioms, dynamical laws, and geometrical theorems, and divest it of many of its complicated mathematical applications, we can hardly help regarding the science as complete.

The controversies and discussions which marked its growth, and which for centuries absorbed the attention of the scientific world, have ceased, and there is left to the present generation a clear, well defined science treating of the forces of nature and the movements which they produce, or are capable of producing, in the ordinary phenomena of force and motion.

Even the last great extension of this science in the development of the true theory of heat, which in its conception and importance was but a fit culmination of the labors of a long line of illustrious men, has now become universally accepted in all its significant features; and although some of the men are now living who were chiefly instrumental in establishing the purely mechanical nature of heat, and in bringing all its manifestations into harmony with mechanical laws, yet this result seems now to have been only the realization of anticipations long looked for, and long before predicted.

No such anticipations now lie before us to excite the zeal of the investigator. There are now no such controversies or oppor-

tunities for discovery to stimulate analytical investigation as marked the last and the preceding centuries. Whatever may be the nomenclature chosen, the forms used, or the classification of subjects, there is no longer any contention regarding the theorems established by Benedetti, Descartes, Leibnitz, Newton, D'Alembert, Bernoulli, and others; and more recently in our own generation, by Clausius, Rankine, Helmholz, and Sir William Thomson.

But while we inherit in all its completeness a science so comprehensive and exact, in its generalizations, as to embrace the movements of the bodies of the solar system on the one hand, and the infinitesimal motions of the molecules of bodies, producing the phenomena of heat, on the other, there is still an unlimited field open for experimental research.

As far as mechanical science is concerned it may be said that we live in an era of *applied science*, — an epoch which has already produced astounding and unexpected results, modifying and changing all the relations of civilized life.

Concerning abstract principles, no kind of knowledge is more widely diffused or universally accepted than that which relates to the subject we are considering. We live and move through the medium of appliances which are but the exemplifications of well known mechanical principles; and with the firm foundations of an exact science to build upon, there is an increasing activity of thought and action, directed, not to the discovery of new principles, but towards new methods, new applications, greater mechanical achievements.

To employ the language of mechanics, there exists at the present time an immense momentum of intellectual force, in this respect, which must either be properly directed to overcoming obstacles in the way of progress, or dissipated in costly failures.

Speculations as to what may be the results of this stored up mental energy, in future developments of the mechanic arts, are unprofitable; but well-directed efforts towards the determination of new applications, or the experimental verification of inductive demonstrations, are seldom without interest; and the more thorough and complete such investigations are made, the greater will be their subsequent value.

There is a wide range of applications of mechanical science to engineering, and in the arts, for which such experimental researches are now demanded.

The recent united appeal of the three principal engineering societies of this country, urging the continuance of appropriations by the United States Government for experiments on the strength and properties of the materials used in engineering structures, is evidence of the want that is felt of a better knowledge of this particular subject.

Among the forces of nature with which we have to deal, there is one class of which the laws have not been subjected to exact determination.

Whatever may be the nature of the forces of gravity and inertia, the laws governing their action are definitely determined; but of the molecular forces which resist the disruption of materials, and which govern their action under the influence of external forces, but little is definitely known; all that is known has been derived from special experiments on specific forms and qualities of materials, which, from the very nature of these materials, are not yet sufficient for general or universal applications. And when it is considered that not only the cost, but the absolute safety, of all engineering structures depends upon this uncertain knowledge, the importance of the question becomes evident. So little has been done in this country in many branches of this subject that reliance is placed almost wholly on the results of investigations made abroad—investigations which were never entirely applicable to the material found here; and which, on account of the rapid changes in the arts of manufacture and fabrication, have become to a certain extent obsolete, or of very uncertain value.

In relation to metallic products, it may be said that a new field of technical inquiry has sprung up, lying between the science of metallurgy and the profession of engineering.

It can hardly be said that the metallurgist is required to answer all questions relating to the properties of the metals in the forms in which they reach the engineer. These properties are determined or modified greatly by the processes of fabrication required to transform the raw materials into the shapes in which they are used. It does not belong to the metallurgist to trace these materials through all the transmutations which they may undergo before being put to use. The very conditions of use often require special treatment, and do certainly enter largely into the inquiries which must be instituted to prove their fitness for special purposes. On the other hand the engineer has not usually the appliances nor the

opportunities, for fully testing the materials he employs. Time, money, and apparatus are necessary, neither of which can usually be spared from his ordinary professional engagements.

For the action of the ordinary materials used in this country reference can only be had to the results of researches by private individuals made on a limited scale, or to more general and systematic, but less applicable, results of experiments instituted by foreign governments.

It is not to be expected that any system of experiments, however carefully conducted, on the properties of the metals, will cover every special case, but there are standard products, and standard processes of fabrication, for which exact determinations of absolute strength, ductility, elastic limits, and other properties, under given conditions of use, may be ascertained.

The rapid changes which are taking place in the production of iron and steel, especially, render such investigations on a scale commensurate with the importance of their useful applications a matter of the highest interest.

It is becoming, in fact, a reproach to our country, that for such investigations, which, on account of the magnitude of the work to be accomplished, can only be undertaken at public expense, so little has been accomplished here. Our engineers are often obliged to refer to experiments made nearly half a century ago for the numerical data necessary for designing the most simple structures.

The names of Hodgkinson and Fairbairn will always be mentioned with veneration, and held in honor, as men whose experimental researches have for more than a quarter of a century formed the groundwork of our structures in iron. But even the youngest engineers become restless under the necessity of constant reference to results which they recognize as belonging to the past and as not wholly applicable to the present. Much useful knowledge has been, from time to time, contributed by our own engineers on this subject, but with some exceptions their results have been generally limited in their range of applications, and not sufficiently comprehensive to become of universal use, on account of want of time and means for prosecuting extended investigations.

It is often argued that it is essential for an engineer to make special tests of the materials he employs in the construction of all important works. While this is undeniably true to a certain

extent, it is also true that an engineer cannot *design* a structure without *assuming* certain data, among which, and the most important of which, are the properties of the materials he proposes to use.

Truly scientific construction demands that all the parts of a structure shall be in harmony, in weight and strength, with the external forces which act upon them, under the conditions of use to which they are subject; whether such a structure be a machine, a bridge, or a public building, the designer must base his construction on these truths, and the want of correct data in any element of the design must entail either loss or damage.

The influence of experimental research in building up the science of pure, or abstract, mechanics shows the impossibility of progress without direct reference of probable laws to the test of experiment.

The dynamic theory of heat remained a speculative question until a few simple and decisive experiments settled all controversies, and opened the way for Rankine and Clausius to develop the new and before unknown science of Thermo-dynamics. We find La Place enunciating this theory in specific terms, and in language identical with that now employed, when merely stating the opposite views of heat entertained in his day; the great mathematician himself upholding until his death the material theory — a theory which is now mentioned only to illustrate, what seems to us in the light of later developments, almost unaccountable ignorance on the part of some of the most learned scientists of the past age. It is true that years of controversy and discussion finally prepared the minds of scientific men for the acceptance of the dynamic theory; but, nevertheless, it required the experiments of Joule to furnish the proof of the theory by showing the absolute identity of heat phenomena, of the most simple kind, with those of ordinary mechanics. And when, finally, through the experiments of Regnault on the specific heats of gases, means were afforded for an independent determination of the mechanical equivalent of a unit of heat, controversy and discussion ceased. These two independent determinations, one by direct experiments and the other through analysis employing the specific heat of air as finally determined by Regnault, were indisputably, when taken together, and especially in view of the importance of the results, the most interesting and important instances of the value of experiments in establishing exact theories in the whole history of science.

The student of mechanics of to-day may not see before him the prospect of brilliant discoveries of new principles, but by patient and faithful labor in the field of experimental investigation, there are still opportunities of adding to mechanical science a knowledge of truths which may have a lasting and beneficial influence in promoting the applications of this science to the Arts and Industries.

I need but refer in this connection to that other series of experiments of Regnault on the elastic force and temperature of vapors, as one of the most signal instances of the value of experimental research, when properly conducted, which can be mentioned. Although by these experiments nothing was added to establish the theory of heat, yet in connection with the universal use of steam as a motive power, and for other purposes, the results have been of vast importance. For accuracy and completeness, these investigations stand unrivalled. They form the basis of all the important applications of the theory of heat to vapors, and thus constitute the foundation for all calculations relating to steam in its various uses. They became not only of immediate use to the practical engineer, but they enabled Rankine, Clausius, and Zeuner to extend the dynamic theory to the study of vapors, and thus to cover all ordinary technical grounds.

Of all the subjects in which experimental research is now demanded, there is none more important than that to which allusion has been already made: the strength and properties of materials used in constructions. It is needless to specify the various kinds of investigation demanded, for our knowledge of the subject, as far as the materials produced in this country are concerned, is in a most uncertain and unsatisfactory state. Boldness of design, and a necessary increase in magnitude of the various parts of structures, are the inevitable tendency of engineering progress. Even in commercial buildings, in which iron scarcely entered as an important element twenty years ago, single cast iron columns have recently been introduced which are required to sustain, permanently, nearly one thousand tons. Similar instances of the use of wrought iron and steel in tension and compression might be given in which for the large cross-sections employed no reliable data for strength is available.

A most important question to be considered where experiments involving large expenditures, expensive apparatus, and much time,

are to be made, is the constitution of the body or commission having charge of such investigations. It is doubtful whether any body of scientific and practical men, however competent they may be, can conduct and control properly the details of such important and laborious work, if they are at the same time burthened by the cares and demands of other private or professional duties. Success in scientific work of this character depends largely on each specific investigation being committed to one person employed solely for the purpose, and whose acquirements and enthusiasm for his subject, need the least amount of advisory aid.

A commission of experts may well direct the character and aim of special investigations, and to some extent the methods to be employed; but without the stimulus of personal responsibility, combined with ardent zeal and special aptitude, of some one who has entire control of details, and who through his constant devotion to one object may reap the principal rewards of his own discoveries, experimental investigations of this kind are apt to end in failure.

Other subjects on which further researches are desirable will readily present themselves. Among these may be mentioned, as of much practical interest, the flow of air and steam through pipes and conduits.

The transmission of power by compressed air, and the adaptation of apparatus for ventilating and heating public and private buildings, require a knowledge of the resistances and other phenomena connected with the flow of air and steam, the available data for which are meagre, and rest upon very incomplete experimental foundations. The complications of this subject, arising from the changes of temperature of the flowing air or steam, and the difficulties thereby caused in attempts to measure quantities of the flowing liquid, render experiments in this direction so tedious and expensive as to operate almost as a bar to ordinary individual efforts.

The importance of the questions involved, especially in connection with sanitary appliances, is, however, becoming greater each year. In this connection also the laws of the transfer of heat, and their technical applications, assume new interest. In many of our large cities works have been erected or are in progress, involving great expense, having for their object the supply of steam for heating and ventilating purposes, and for power, by transmitting

the steam in underground pipes throughout large districts from a few central stations.

These undertakings present numerous problems which have not heretofore been encountered in engineering operations. Correct data concerning the laws of the flow of gases and vapors, and the transfer of heat, constitute an important element in many of these problems. While compared with similar data for the flow of liquids, it can hardly be said that the knowledge available to the engineer is reliable or satisfactory.

In the construction of public buildings, private dwellings, and factories, the necessity of including in the first design complete arrangements for warming and ventilation, has of late years been forced upon architects and engineers by a public demand for healthful arrangements; and all the scientific and technical knowledge that can be brought to bear on the subject is desirable. In this direction but little has been accomplished in this country, except in connection with private practice. For information that cannot be obtained from this source, recourse must be had to foreign works, some of which, notably those of Peclét and Morin, may be taken as examples of the great value of such experiments as were made by these distinguished experimenters and authors.

It would be impracticable to present in detail all the principal subjects of practical mechanics in which more exact knowledge is needed in technical applications. There is a universal activity at the present time, everywhere, towards overcoming obstacles to what is called progress.

The demand is constantly for greater achievements. The public calls imperiously for greater speed on railways and steamships, and the engineer must meet the demand. Greater speed requires heavier rails, heavier engines, larger ships, and stronger machinery. But the same imperious public also insists on absolute safety, and reasonable cost. A train must fly at high speed across long bridges, built at the least possible expense, but woe to the engineer if a calamity occurs. No explanations are received; the public will accept no part of the blame.

This increasing ambition for something faster, grander, more convenient, or more healthful, brings with it increasing risks and responsibilities to professional men. The members of the engineering profession, especially, have to assume great risks, and it is not unreasonable that they should ask public aid, as our engi-

neering societies have done, to arrive at a kind of knowledge that can be procured in no other way than through public aid and expense, and without which their best efforts may often result disastrously. If we include in these general propositions other branches of technical science, especially Chemistry and Physics, it would not be hazarding too much to say that the continuance and permanence of what is usually called modern progress depend greatly on a general recognition of the importance of this kind of scientific research.

There is one field of research which alone may well occupy the attention in this respect of the best minds of the age, and which ought to receive the most generous public support. That field is sanitary science, or sanitary engineering. Wordy controversies can never satisfactorily settle questions of ventilation, which involve force and motion, the heating of air and its conduction and distribution; nor questions of poisonous emanations or solutions, in air or water, nor questions of house and street drainage. Connected with these subjects there are abundant opportunities for systematic experiments, not only in the domain of physics and chemistry but of mechanics.

We cannot expect to rival the nations of antiquity in architectural achievements, although it would seem that lavish expenditures on monumental buildings most readily satisfy public ambition even in this age; but it is to be hoped that the generous impulses which have begun to manifest themselves in the direction of sanitary improvements may absorb a full measure of this ambition and direct it towards the most perfect provisions for the health of communities, so that social life for the poor as well as for the rich may rest on the highest possible plane of physical as well as mental and moral excellence. A small share of the millions expended by the people in the architectural masonry and decorations of public buildings, would, if properly devoted to mechanical, physical, and chemical researches connected with sanitary engineering, accomplish much towards this end.

There is undoubtedly much force and wisdom in the suggestive remark of one of the most distinguished sanitary engineers of England, that "if he were permitted to build the homes of a people, he would not care who should write their songs, or make their laws."

There is a branch of mechanics which has from time to time

excited considerable public attention, but which, from its not presenting features of great public utility, has not been ranked among those having a commanding importance.

It is only necessary to mention *animal mechanics* to bring to mind the beautiful experiments of Marey in determining the movements of animals in the ordinary processes of locomotion. A study like that of Marey of the mere motions of the locomotive appendages of animals — the curves described by the extremities in the operations of flying, walking, and swimming — must always remain barren of useful results unless connected with the dynamical features of the problems involved. These Dr. Haughton, of Trinity College, Dublin, has supplied, by a most interesting treatment of the subject, and by careful experiments, extending over a period of ten years.

Muscular force is so related to life that it appears almost to furnish the only unmistakable evidence of vitality. When the involuntary and voluntary muscles of the animal system cease to act, life can no longer be said to exist. Dr. Haughton has discussed the whole subject of muscular forces not only in their relations to the skeletons through the medium of which they act, but in their properties of strength, endurance, refreshment, and fatigue, in such a manner as to throw new light on this interesting subject, and to suggest its real useful aspects in connection with the laws of health and physical development.

The conformity of the phenomena observed and the principles established, in regard to movements connected with animal life, with the laws of ordinary mechanics, is complete. The four elementary machines, which constitute either singly or in various combinations the bases of all artificial machinery, are not only found to be represented in the various kinds of animal locomotion, but, except perhaps in some of the very lowest orders of animals, no other means of locomotion can be discovered.

In relation to the expenditure of energy, it appears to have been demonstrated also, that the sources of muscular, and probably of nervous energy, are, as in the ordinary expenditures of force, subject to exhaustion at rates which increase in a high degree with the rapidity with which work is performed.

The space which a steamship can traverse, with a given amount of coal for consumption, varies inversely as the square of the speed; and the number of days required for the exhaustion of the fuel,

inversely as the cube of the speed. Similar laws are applicable, as Dr. Haughton has shown, to human exertions and furnish scientific and mechanical reasons, why the prolonged, intense nervous activity of business life, so much complained of in these days, results in premature and permanent disabilities. Doctor Haughton was the first to demonstrate experimentally the laws of fatigue and refreshment of muscular action, and to suggest, at least by very natural inferences, that there are limits of exertion within which alone these processes may be expected healthfully to balance each other.

In one sense all great works, successfully executed, furnish data and precedents for the future. Each one reaches out beyond its predecessor, the result being the gradual building up of an art, an industry, or perhaps a profession.

In exceptional cases a single construction, or a single exploit, revolutionizes practice. Two episodes of our late war present signal examples in this respect. The conflict between the Monitor and Merrimack in Hampton Roads furnishes the first real experimental test of the invulnerability of iron-clad vessels, and the efficiency of the turret system devised by Ericsson; while the demolition of Fort Pulaski by General Gilmore, by batteries placed at one and two miles distance, destroyed, in a day, all faith in defensive works of masonry, which up to that time had been regarded as the bulwarks of seacoast defence. History furnishes no instance of so complete and universal a change in practice caused by one gigantic experiment occupying hardly more than twenty-four hours. The echoes of Gilmore's guns had scarcely died away before orders were issued from the war offices of all nations having seacoasts to defend to suspend work on masonry fortifications.

These two events, the conflict of the Monitor and Merrimack, and the reduction of Fort Pulaski, were the beginnings, however, of a contest which has lasted to the present time — the contest between rifled ordnance and defensive armor. In this war of force, of impact and resistance, if we take into account the cost of armored vessels, hundreds of millions of dollars have been expended.

In the course of ordinary events, however, isolated results do not furnish the most useful collection of facts; there are few branches of art or industry, involving mechanical appliances,

which may not be advanced by investigations systematically undertaken, and which are made through a careful observance of all phenomena and conditions to bring out general laws.

It will not be considered inappropriate in connection with this subject to make a brief allusion, and render tribute to the works of one who, though not now living, may be regarded as having been among the foremost investigators of the present age in the department of Mechanical Science. No name is more frequently mentioned, wherever the English language is spoken, as authority in matters relating to applied mechanics than the name of RANKINE. His original investigations in founding, and building up, the science of thermo-dynamics, were sufficient to establish his fame; but the peculiar fascination of his works, to the engineer, lies in the wonderful scope of his genius in the solution of problems of applied mechanics in every branch of engineering, and the remarkable skill and perception with which he introduced experimental data into his researches; thus giving to his deductions the most unerring scientific exactness combined with the nearest approximate practical solutions.

Unlike the writings of many scientific authors, Rankine's voluminous works will not bear reduction or condensation. Conciseness of style and the shortest modes of demonstration are their marked characteristics. Every paragraph, and almost every line, seems to contain the expression of an idea, a valuable description, or the enunciation of a principle.

As an investigator he appears never to have touched a subject without throwing new light upon it, either by his mode of treatment, the discovery of new principles, or in extending its practical applications. Although not an experimenter himself, no investigator has made more constant use of well established results of experiments made by others, or has been more skilful in bringing useful rules of practice out of the combinations of such results with exact theories. He has left to the present generation the most complete exposition of the principles of the science of applied mechanics that can be found in the English language.

England has not yet done full justice to the memory of one whose labors in science ought to be regarded as having added to her national fame, and whose practical expositions have contributed so much to the progress of all nations in the Mechanic Arts.

PAPERS READ.

On a Newly Discovered Absolute Limit to Economical Expansion in the Steam Engine and in other Heat-motors.

By Robert H. Thurston, of Hoboken, N. J.

[ABSTRACT.[1]]

It has been universally assumed by engineers, and probably by all physicists familiar with the theory of the steam engine hitherto accepted, that the best ratio of expansion, whether for maximum efficiency of fluid, of engine, of capital, or of plant,[2] increases with increase of steam pressure without limit, and that such ratio may be indefinitely increased with decrease of the ratio of back-pressure for any one kind of engine, notwithstanding the fact that the value of the ratio of expansion is modified by variation of the conditions of working, even where the ratio $\frac{p_b}{b_1}$ is the same. The writer proposes to show that, in every actual engine, in every engine operated under the conditions of real work and of usual practice, there exists a limiting value of any of these "ratios of maximum efficiency" beyond which it cannot be economically raised even with an infinitely elevated boiler-pressure. It will be further shown that this "absolute limit" may be readily, and probably often is, passed in everyday practice, that in the usual forms of steam engine an absolute limit exists within or not far beyond the customary working range of expansion, beyond which expansion cannot be carried with economy, however high steam pressure may be adopted; in other words, with infinite pressure, the economical value of the ratio of expansion will be found often not merely finite, but sometimes probably within the limits of familiar practice.

In earlier papers[3] the writer has shown that the conditions

[1] This paper is published in full in the Journal of the Franklin Institute, Sept., 1882.

[2] *Vide* a paper "On the Several Efficiencies of the Steam Engine," etc., Journal of the Franklin Institute, May and June, 1882; *Transactions of the American Society of Mechanical Engineers*, 1882.

[3] Journal of the Franklin Institute, May, 1881, February, May, and June, 1882; *Transactions of the American Society of Mechanical Engineers*, 1881-2.

under which engines are worked in actual practice differ in a very important degree from those assumed in the simple thermo-dynamic theory of heat engines, and, especially, that the unjacketed steam cylinder, which is usually assumed to be of non-conducting material, is to such a serious extent a reservoir and a transferrer of waste heat that the efficiency of the engine is greatly and sometimes enormously reduced.

The equations which would give values of the ratios of maximum efficiency for the ideal "perfect engine," therefore, do not apply to real engines," and the author has deduced[4] modified equations, which are at least approximately correct for real practice. He has also produced true "curves of efficiency" from real engines, which were found to be practically the same in form and location, to be practically identical in character, with those inductively obtained, so far as the two could be compared.

"THE GENERAL EQUATION OF ALL STEAM ENGINE EFFICIENCIES," is

$$\frac{1}{E} = \frac{A v_2 + B v_2 r^{-1}}{2R\left(b p_1 v_2 \dfrac{n r^{-1} - r^{-n}}{n-1} r^q - p_b v_2\right)} \qquad (A)$$

which becomes a minimum, and makes the *commercial efficiency* of an engine doing the required work a maximum when, to obtain r, we have made

$$r^q + \frac{q}{M(q-1)} r^{q-1} - \frac{q-n}{n(q-1)} r^{q-n+1} \cdot \frac{q-n+1}{Mn(q-1)} r^{q-n} = \frac{n-1}{Mnb(q-1)} \frac{p^b}{p_1} \qquad (B)$$

The ratio $\frac{p_b}{p_1}$ is the quotient of the total back pressure by initial pressure, all useless resistance being included in p_b. A is the measure of annual costs, variable with size of engine, B of costs variable with quantity of steam demanded, v_2 the volume of steam-cylinder, r the ratio of expansion, R the revolutions per annum, b the proportion of steam in the cylinder when $r = 1$, p_1 is initial steam pressure, n is the exponent of the familiar equation of the steam line, $pv^n = \text{const.}$, and r^q is the rate of variation of losses with increase of r. $M = \frac{A}{B}$.

When instead of M, N is inserted in the equation, N being the quotient of *all* annual expenses, independent of fuel supply, by

[4] "On the Several Efficiencies of the Steam Engines," etc.

all such expenses variable with fuel and steam supply — the expression determines the ratio of expansion at "maximum efficiency of capital invested in a *given plant*."

When M or N is made zero, the expression reduces to:

$$\left(r^{q-1} - \frac{q+1-n}{q\,n}\, r^{q-n}\right) \div \frac{n-1}{b\,q\,n} = \frac{p_b}{p_1} \qquad (C)$$

and gives the ratio of expansion at which maximum "duty" or "efficiency of engine" is attained.

When $b = 1$ and $q = 0$, the case becomes that of the ideal engine working the fluid in a non-conducting cylinder, and these equations become, for efficiency of capital and of engine,

$$r^{-n} - \left\{\begin{matrix} M \\ \text{or} \\ N \end{matrix}\right\} \frac{n - n\,r^{1-n}}{n-1} = \frac{p_b}{p_1} \qquad (D)$$

$$r^{-n} = \frac{p_b}{p_1} \qquad (E)$$

Studying these equations, it will be seen that, in all except the last (E), it is possible to find finite values of r such that their left-hand members shall reduce to zero; as in them n is nearly always equal to unity; q varies from $q = 0$ to $q = 0.3$ in good practice, and b usually ranges between $b = 0.8$ and $b = 0.9$; M or N is usually between 0.02 and 0.15, and the form of the function is such that the first member may always be made to disappear for some finite value of r. Then we shall have in (B)

$$r^q + \frac{q\,r^{q-1}}{M(q-1)} - \frac{(q-n)r^{q-n+1}}{n(q-1)} - \frac{q-n+1)}{Mn(q-1)} = o; \qquad (F)$$

and in (C)

$$r^{-n} - M\,\frac{n - n\,r^{1-n}}{n-1} = o$$

$\frac{p_b}{p_1} = o$; $\frac{p_1}{p_b} = \infty$ and the value of r, at which this condition is obtained, constitutes an "absolute limit," for the case taken, beyond which expansion cannot be carried economically, even with steam increased to infinite tension; beyond this point $\frac{p_b}{p_1}$ becomes negative, indicating the assumption of impossible conditions.

Examining equation (E) we find no such limit; the sign of the first member remains positive for all values of r, and can never become zero for a finite value of that quantity. We are thus taught that an important difference exists between the *ideal* engine, with its non-conducting cylinder, and the *real* engine working steam in

a metallic cylinder, as well as between the case of maximum efficiency of engine and that of maximum efficiency of capital. In the cases of maximum efficiency of fluid and of engine for the ideal perfect engine, equation (E), only, is it true that indefinite increase of steam pressure permits indefinitely increased expansion. In all other cases an absolute limit exists, fixed for each case, beyond which expansion cannot be economically carried.

The above equations for real engines are approximate, and are practically exact — the values of the constants being determined with accuracy — within the range met with in practice, and the conclusions here deduced are, therefore, correct, although the exact values of the constants, or even the precise form of the variable function, r^q, here may not be fully ascertained.

Should it seem desirable, it is easy to deduce the same conclusions — and thus to confirm, by independent proof, the above proposition — by the examination of the "Curves of Efficiency" of ideal and of real engines.[5]

Thus it is seen that, while the ratio has no limit for the ideal case, it has such a limit for the real engine, and that this limit may be found at a low ratio of expansion. The writer has made this comparison for the U. S. steamers "Michigan," "Georgiana," and "Bache," for which three cases the real curves have been obtained by him,[6] and finds that, these curves remaining unchanged,[7] it is impossible economically to increase the ratio of expansion in such engines beyond three, five, and ten respectively, even with unlimited steam pressure; *i. e.*, even when $\frac{p_b}{p_1} = 0$.[8]

We may therefore conclude:

(1) That in all engines there exists an "absolute limit to the economical expansion of steam," whether considered with reference to efficiency of fluid, of engine, or of capital, which limit can not be passed, whatever pressure of steam may be carried up to the point of cut-off.

[5] With such curves the ordinates measure the work done at various ratios of expansion in any given engine, by quantities of steam proportional to the abscissas.

[6] *Vide Transactions of the American Society of Mechanical Engineers*, 1882; JOURNAL OF THE FRANKLIN INSTITUTE, June, 1882.

[7] The form and location of these curves at very high pressure would undoubtedly be somewhat altered, but it is evident that there must still be found this absolute limit so long as condensation increases with extended expansion. The writer is inclined to believe that the form of the curves will be substantially as shown, however, at any attainable pressure.

[8] The "Michigan" has an unjacketed condensing engine, using saturated steam; the "Georgiana" a similar class of engine, with superheated steam; the "Bache's" engine is jacketed and compound.

(2) That this limit is found at higher ratios of expansion as the type of engine is more efficient, but that the limit is indefinitely removed only in the ideal engine, and then only as affecting the ratios of expansion at maximum efficiency of fluid and engine.

(3) That this limit is found at a small value of the ratio of expansion in ordinary engines, and therefore may be readily passed in every-day practice.

The limit is not far from three in the common, unjacketed condensing engine, four or five in the same engine using superheated steam, and ten or twelve in the ordinary compound engine.

(4) It is evident that the general propositions of this paper are true of all heat engines having fluid working substances, whether vapors or gases worked in metallic cylinders.

TRANSVERSE STRENGTH OF LARGE SPRUCE BEAMS. By GAETANO LANZA, of Boston, Mass.

[ABSTRACT.]

THE moduli of rupture and the moduli of elasticity of timber, given in our text-books and engineers' hand-books for practical use in construction, the former for determining the breaking weight of beams, and the latter for determining their deflections under given loads have, in almost all cases, been deduced from experiments on small beams, not at all comparable in size with those in common use in building.

While such a course is, to-day, recognized as misleading by our best engineers, it has been and is still followed very extensively, because, with a very few exceptions, we have had no experiments on the strength or deflection of full size wooden beams subjected to a transverse load.

Again, no experiments, as far as I know, have been made on the large scale on the strength of such timber joints as are used in framing, where mortises and tenons are employed, or where notches are cut, and we have had no experimental basis from which to deduce their strength, so that the loads which have been supposed or assumed to be their breaking loads are probably very far from the true breaking loads.

In view of these facts, and of the practical importance of experiments on full size pieces to the engineer, the architect, and the

builder, I have been so desirous of having, at the Massachusetts Institute of Technology the means of carrying them on, and of giving my students the opportunity of performing them, that I have had constructed a machine capable of testing a beam transversely loaded with a load not greater than 50,000 pounds, provided the beam has a length not exceeding twenty-five feet.

By its means we are also able to test most of the framing joints used in practice. The results actually obtained by testing spruce beams are given in the following table:—

Number of test.	Width and depth in inches.	Distance between supports.	Manner of fixing the ends and loading.	Breaking weight in pounds.	Modulus of rupture in lbs. per square inch.	Trautwine's form. Central breaking weight of beam 1" square and 1' long supported at ends.	Barlow's form. End breaking weight of beam 1" square and 1" long, fixed at one end only.
1	2 x12	6' 8 "	Framed at ends.	6,304	.....	.....	.....
2	4 x12	6' 8 "	Header.	10,338	.....	.....	.....
3	2 x12	15'	Load at middle.	5,794	5,432	302	906
4	2 x 9	6' 7.5"	" "	7,227	5,320	295	887
5	2 x12	15'	" "	5,586	5,237	291	873
5 (a)	2 x12	7'	" "	8,982	.....	.....	.....
6	2.75x 9	6' 8 "	" "	7,586	4,082	226	680
7	3 x 9	4'	" "	11,086	3,285	183	548
8	3 x 9	10'	" "	6,086	4,508	250	751
9	3 x 9	15'	" "	5,086	5,651	314	942
10	3.88x12	20'	" "	6,586	4,258	237	709
11	2.5 x13.5	10'	" "	9,585	3,787	210	631
12	3.75x12	16'	Load 4.5' from one support.	7,585	3,271	182	645
13	3.75x12	16'	Ditto mortised.	9,085	.....	.....	.....
14	7 x 2	7'	Load at middle.	1,944	8,748	486	1,458
15	1.75x 6.75	7'	" "	4,785	7,562	420	1,260
16	3 x 9	6' 8 "	" "	9,985	4,931	274	822
17	3 x 9	6' 8 "	Load distrib. at 4 points, 16" apart.	16,744	4,961	276	827
18	3.9 x12	16'	Load 4.5' from one support.	12,585	5,218	289	869
				13\59.986 / 4,610		256	768

As to the quality of the timber, it was, with the exception of Nos. 14 and 15, all common merchantable stock, its average modulus of rupture being 4623 pounds per square inch, the largest being 5651.

Nos. 14 and 15 were cut in 1877 and had been seasoning on the wharf since that time till they were tested in April, 1882.

Nos. 1, 2 and 13 are tests of framing joints, and hence from them we cannot determine a modulus of rupture. No. 5 (*a*) was a piece sawed from one end of No. 5, after this had been broken; hence, from this no modulus has been deduced.

The question must now be answered how these moduli compare with those commonly given in our text-books and engineers' handbooks.

	Max.	Min.	Mean.
Hatfield gives	12,996	7,506	9,900
Rankine gives	12,300	9,900	11,100
Laslett gives	9,707	7,506	9,045
Trautwine gives			8,100
Rodman gives			6,168
These tests have given for merchantable stock	5,651	3,271	4,623

Hatfield's, Laslett's, Trautwine's and Rodman's figures are from their own experiments, and, as is evident from the above table, they differ very considerably. Trautwine advises for practical use to deduct one-third, on account of knots and defects, hence after making this allowance he would use 5400. My own figures would give this only for the best of the stock in common use, and in the case of knotty and poor timbers would give much lower values; I obtained as low as 3271 and an average of 4623.

It will be observed that although Rodman's result is not at all as small as mine, it is smaller than any of the others; this is to be accounted for by the fact that the pieces which he experimented on were $5\frac{3}{4}$ inches deep, $2\frac{7}{8}$ inches wide, and 50 inches between the supports, being larger than those generally employed by other experimenters, though not at all as large as those used in practice.

Only three tests have been made to determine the modulus of elasticity, and these have given respectively as follows:—

No. 3	1,237,215
No. 4	1,067,893
No. 6	938,453
	3) 3,243,561
Average	1,081,187

The machine has only been at work about two months, and hence no more work has yet been done, but these and other series of tests will be made in the future, and it is hoped will furnish us some more information of practical importance.

NOTE.—Since the above paper was delivered a much larger number of tests have been made, and a great deal more information obtained on the subject, a part of which was published in the Franklin Institute Journal for February, 1883.

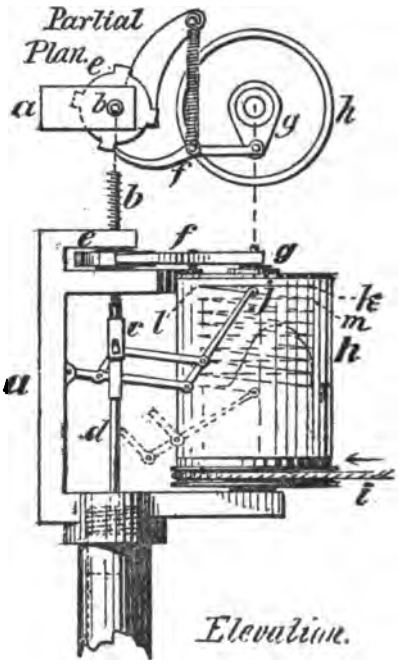
A METHOD OF CUTTING SCREWS OF INCREASING PITCH. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

A SCREW whose pitch increases uniformly from one end to the other may be cut by the following method.

Mount a suitable cutter, say one formed like a short reamer, with teeth also on the end surface (a "Brown and Sharpe" cutter for the teeth of wheels could also be used), in a milling machine, and on the bed of the same fasten two journals, so arranged that the blank for the screw may either revolve or slide in them without shaking. The screw-blank as it rests in these journals must, of course, be in the proper relation to the cutter, so that if it were moved in the direction of its axis, a groove parallel thereto would be milled in the blank, and if it were revolved a groove would be milled around the blank, while by a suitable combination of these

motions any desirable screw would be milled. Suppose now that by some means, we get, say, a quarter of a turn of a screw of any form milled, and that we then attach to the bed of the machine a pin, of the same size as the cutter and somewhat in advance of it, to act as a nut. The end of this pin must fit in the groove in exactly the same way as the cutter itself, so that by turning the screw-blank it will feed past this pin as if it were a small portion of a nut. It will now be seen that by arranging a feed to revolve the



screw-blank we can make the latter screw forward past the fixed pin, and that, therefore, the cutter will extend the groove indefinitely. It is also clear, that the form of the original portion of the groove (the form of the section of the groove is not the form meant) will be repeated throughout the screw.

In order now to make a screw whose pitch shall increase in arithmetical progression, we have only to make the original portion in accordance with this law, and then to arrange a second feed (the ordinary feed of the milling machine) by which the bed with

all that is fast to it may be uniformly moved a certain distance in the direction of the axis of the screw-blank for every revolution of the latter.

By reversing this latter feed, on passing the middle of the blank, a screw-cam can be cut, which shall give an oscillation of its nut, and by suitable variations of this feed many other forms can be cut.

Indicator Attachment for High Speeds.

By J. Burkitt Webb, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

The steam-engine indicator, in the form given it by Watt, is essentially a low-speed instrument, and momentum and friction will ever be present to vitiate its indications for the high speeds coming into use. A new principle must, therefore, be adopted, which shall make the action of the instrument independent of the speed of the engine.

This new principle has been embodied in an instrument quite different from the ordinary indicator, and also in an attachment to be applied to the latter; as the former contains, besides the essential and radical improvements, several conveniences, the shorter and simpler description of the latter will be given.

The new principle may be thus explained:—in the ordinary instrument we have a positive, horizontal, oscillating motion of the paper, coinciding with that of the piston, and a non-positive, or free, vertical motion of the pencil dependent on the steam pressure in the cylinder; in the new form of instrument the first motion is the same, but it is made more certainly positive; for the second motion, however, is substituted a positive vertical-feed of the pencil, dependent on the first motion. These two motions form on the paper what we have called the "blank-diagram," which will, from the manner of its formation, and in the figure, be seen to consist of a parallelogram of zigzag "pressure lines," k, l, m, etc., such as would be made by oscillating (instead of continuously revolving) a chronograph cylinder. This arrangement evidently eliminates

the troublesome effects of momentum, and the blank-diagram is perfectly accurate.

The volume and pressure curve is formed upon this blank-diagram by a third or "registering motion," which introduces what we may call a "chronograph" jog in the pressure-lines as they cross this curve. This motion is dependent, of course, on the steam-pressure, and being exceedingly small it allows of the employment of a diaphragm, instead of the ordinary piston and cylinder, thus eliminating the injurious effects of friction and making it possible to get rid of the cock and of most of the waste room between the indicator and the steam-engine cylinder.

We think it no disadvantage that this new form of instrument requires a number of strokes of the engine for the completion of a diagram; in fact, in the ordinary instrument no attempt is made to confine its action to a single stroke.

Experiments to Determine the Strength of Cylinders with Domes Attached. By Samuel Marsden, of Saint Louis, Mo.

[ABSTRACT.]

These cylinders were made in imitation of a locomotive boiler with a dome, and the object of the experiments was to ascertain one of the ways in which such a boiler gives way under pressure. Five cylinders were tested, they were about four inches in diameter, and their domes varied in size. They were of lead and the domes were soldered to them; this material was chosen on account of its uniformity. The holes through the walls of the cylinders, which connected their interiors with those of the domes were concentric with the latter, but not in all cases of the same diameter, and this proved to be the point of main importance. When this hole was made as large as the dome the cylinder and its dome burst by cracking in a plane passing through the axis of both, the crack starting on both sides of the dome, at the angle between dome and cylinder; but when this hole was made small the cylinder burst on the side opposite to the dome, much the same as an ordinary cylinder would have done.

AËRIAL NAVIGATION. By JOS. DE L'ETOILE, of Ottawa, Canada

[ABSTRACT.]

THIS paper was a review of the subjects of atmospheric currents, electricity and gases with a view to practical aërial navigation by means of balloons: to take from the air the means to navigate through the air, and thus do away altogether with the ballast and all such dead weight material which has been and still is the real cause of the little or no progress at all, having been made with regard to the guidance of balloons. The balloon should take the form of a fish and be provided with a propeller, a rudder, an air compartment, gas and air pumps or exhausters, electric battery, electric meter, and two safety valves. The balloon should be made to rise slowly and to descend with ease to the earth. All danger from electric flashes coming in contact with the escaping gas through the envelope of the balloon has been obviated by using material which prevents such escape. The above plan was tried, in the year 1869, and it worked satisfactorily. When the ensemble of this new aërial vessel shall be put to a test, it will, no doubt, prove as important to the aëronaut as the steamboat and locomotive are to the engineer.

Atmospheric currents vary at different altitudes, and by ascending through high atmospheric regions, aëronauts cannot fail to meet with favorable winds. Observations for scientific purposes, taken during ascensions in balloons, have hitherto been but imperfectly made. Globated balloons usually rise in an oblique direction and under the combined influence of the direction of the wind. The change of temperature in the atmosphere in a vertical ascension could not naturally be the same as that met with in an oblique direction and sometimes at twenty or fifty miles horizontally from the starting point, as the atmosphere above New York could not be the same as that met with above Boston at the same altitudes, at the same hour and minute, and under the same influence of aërial current. For want of practical steerable balloons, the regions of the two poles of our globe are yet unknown to geographers. The different atmospheric currents at certain altitudes, and at different places in a given hour and minute, are still a puzzle to meteorologists. The capacity of telescope glasses

has not as yet been ascertained at different altitudes and under different atmospheric conditions.

Astronomers seem to have overlooked the fact that a glass cannot be as true to the eye when looked through on the surface of the earth, as it would if above atmospheric electric currents. European and American governments have, without forethought, sent brave men, on board of ships which cost millions of dollars, to the arctic regions, and what has been the result? Of course, great loss of lives and the sinking in the deep of all such valuable property. Let any one beg government aid for the minimum sum of say, four thousand dollars, which would be sufficient to meet the expenses of building a steerable aërial ship; what would probably be the answer?

THE FUTURE OF THE BALLOON AS A PRACTICAL MEANS OF AËRIAL TRAVEL. By W. H. LYNCH, of Danville, P. Q., Canada.

[ABSTRACT.]

THE two systems of aërial navigation, or rather the system of actual aërial travel by balloon and the theory of aërial navigation by utilizing the flying principle, are quite opposite.

In the flying machine the propeller or wings are required to act upon the air at once to lift the dead weight of the machine, and to propel it or direct its course. The balloon, on the contrary, is itself a lifting power, lifting itself and carrying foreign weight that may be a force to direct or propel it.

We do not forget that the self-lifting machine is in one respect at a disadvantage with the inert or dead-weight machine. To be lighter than air means to be bulky, and to offer in motion greater resistance to the air, while what is heavier than air is more compact, and its motion more easily caused or directed. This one advantage of the flying-machine over the balloon counts, however, but little so long as it cannot both lift itself and carry the little extra weight of the force required to propel it. The balloon will carry no mean weight besides its own. It will carry, besides possible passenger or freight weight, the extra weight of a force that may direct or even propel the whole.

In answer to the question from which principle may the best results be expected, we may note that the dead-weight principle has given no promise that we shall get from it, alone at least, any practical result. The balloon principle, on the other hand, has already given us so much that no one may positively negative the opinion of Glaisher that it is the "first principle of some aërial machine that remains to be suggested." The peculiar disadvantage of the balloon, that of bulkiness, may be minimized, and the flying principle itself may doubtless be utilized to its full value in its application to the direction, and perhaps actual propulsion of the balloon.

Is the general ridicule of efforts to utilize the balloon a reason to believe that it has no practical future? We think not. Wonderful has been the progress in means of travel, on land and water. Our cars and boats are palaces for comfort and models of strength. Steam-engines are of giant power and perfectly under arm control. Road-beds are built with wonderful engineering skill, through rock or over rivers. Water ways are charted and known, to be followed easily by night or day. Yet everyday's newspaper has its record of accidents and disasters. The time is doubtless approaching when we shall travel with an approach to absolute security and safety. Still greater achievements of science and ingenuity will make even swifter travel more safe, as well as more comfortable. Explosions on river boats once so common are even now quite rare. Broken rails which were an everyday occurrence when rails were iron, are hardly known to-day when rails are steel. Collisions will be less frequent when our roads shall have double tracks. Running off the track may degenerate to quite a safe excitement by the possible common use of inexpensive girders, which are suggested by those already used on the New York elevated roads. The risk of wreckings of trains will be minimized by improvements in switches, in our system of crossings, etc. All this shows that even in surface travel the great progress already made leaves much to be desired, and the accomplishment of much to be expected. In aërial travel there has been almost no progress, and instead of hopeful expectation, the question is raised whether attainment in this direction is possible. We have only to take the history of ballooning, showing, as it does, the unscientific character of most efforts made to solve the problem of aërial travel, to negative all such assumption, and to encourage the expectation that the future has great results in store in the line of progress.

What the compass was to the mariner, making possible the discovery of a continent; what the electric telegraph was to the railway, making possible the running of trains on single tracks, modern science and invention may be to balloons. The batteries for storing electric energy, the knowledge being gained of the properties of gas, the remarkable and great advance in meteorological science, are all particular factors in the solution of the problem. The aëronaut of the future, like the driver of the steam-engine of to-day, must have his power at his hand, his gauges under his eye, and his route, with its advantages and dangers, in his mind. He will not depend upon the unscientific helps of spare ballast and gas to be utilized only by an uneconomic waste, or even upon the long ropes that dangle over the chimneys of houses or toy with the crests of icebergs. With the application of his abundant and ever-ready power to a suitable propeller he will in some measure direct his course at will. With a knowledge of air currents and analogous matters, he will move up or down to suit his need, and either seek the help of wind or evade its power.

When the problem shall be solved it will doubtless be through the acceptance and improvement of the balloon principle. When the balloon, that has already proved itself a carrying power of no mean value, has had turned upon it all the helps that science can give to utilize its possibilities, the difficulties of aërial navigation may vanish.

That the problem will be solved, men sanguine of any great future improvements and changes will not question. The fact of no advance for a century does not, to the minds of thinking men, annihilate the possibilities in the full application to any problem of modern science, ingenuity and skill.

TITLES OF OTHER PAPERS READ IN SECTION D.

THE PERMEABILITY OF THE LININGS OF HOUSE-WALLS TO AIR. By Thomas R. Baker, of Millersville, Pa.

ST. LAWRENCE BRIDGE AND MANUFACTURING SCHEME. By F. Foster Bateman, of Montreal, Canada.

SECTION E.

GEOLOGY AND GEOGRAPHY.

PAPERS READ.

The Glacial Flood of the Connecticut River Valley. By C. H. Hitchcock, of Hanover, N. H.

[ABSTRACT.]

In 1878, Mr. Warren Upham's report upon the "Modified Drift of New Hampshire" appeared in print. He sketched in full, with maps and sections, the terraces and the kame of the Connecticut valley in New Hampshire and Vermont.

The phenomena were explained by supposing the various deposits to have been accumulated by the flood, attendant upon the melting of the Quaternary ice-sheet. The kame was formed first, and then the higher terraces, which constituted part of an enormous flood plain. When the ice was gone the waters diminished in bulk and the lower terraces were formed. Two kinds of terraces were specially noted: first, the normal highest lateral terraces deposited by the Connecticut flood; second, the deltas of the tributaries to the main stream — the latter being very commonly higher than the others.

Three years later, Prof. J. D. Dana went over the same ground with Mr. Upham's report in hand, in order to procure the additional facts required to estimate the size and velocity of the ancient river. With spirit levels he verified the accuracy of the report, and estimated what the velocity of the current must have been upon the basis of his estimate of the width and depth of the flood. For the country between Massachusetts and Wells river the mean width was thought to average 4,000 feet, and the mean depth 140 feet. The formula for the calculation of the velocity was that given by Humphreys and Abbot.

The mean velocity of the flood from Haverhill, N. H., to Middletown, Ct., must have been over twelve miles per hour; allowing

for retardations of various kinds, the rate may be assumed to be ten miles per hour,— this velocity must have been far too great. A study of the materials actually transported indicates an actual velocity of three to four miles per hour. In order to bring the calculated and observed velocities nearer together, it is suggested that the land pitched to the south less than now; for marine fossils occur at increasingly greater elevations from south to north, and consequently the ocean must have been higher in the north. At Montreal it stood 520 feet higher than now; and by proportioning the height to the distance, the Connecticut river at Haverhill must have flowed over a floor 120 feet nearer the ocean level than it is now. By using these figures Professor Dana estimated that when the flood was at its maximum the rate of velocity must have been as much as eight miles per hour. This he allows is much too great, but he does not state how the velocity can have been reduced to the proper limit.[1]

In the statement of the facts, exceptions are taken to Upham's doctrine concerning the method of determining the height of the water. Attention is called to these discrepant views and to the presumption of the correctness of those advanced by Upham, because they reduce the magnitude of the flood and consequently the velocity of the stream, and thus better harmonize our theories with the facts. Dana bases his estimates for the depth of the flood upon the present altitudes of the deltas of the tributary streams. Upham claims that the height of the tributary deltas should not be taken as the measure for the main stream because it is in excess of the truth. The height of the usual lateral terrace between the tributaries is regarded as the more satisfactory measuring rod. This deposit was confessedly made by the main stream.

The following table, as compiled from the sources cited above, will show the differences required for the height of the water according to the diverse theories. Only that part of the river's course between Barnet and Massachusetts is considered.

[1] See Amer. Jour. Sci., III, Vol. XXIII, Feb., March, May, 1882.

	River above Ocean.	Highest Normal Terrace above river.	H. N. T. above Ocean.	Deltas above river (Upham).	Deltas above Ocean (Upham).	Deltas above river (Dana).	Deltas above Ocean (Dana).	Deltas above H. N. T.
Barnet.....	452	166	618	223	675	225	677	57 U. 59 D.
Wells river.....	407	123	530	253	660	253	660	130 both
Haverhill.....	392	83	475	238	630	263	655	155 U. 180 D.
Piermont.....	387	78	465	113	500	258	645	35 " 180 "
Orford.....	383	52	435	177	560	247	630	125 " 195 "
E. Thetford.....	378	167	545	157	535	232	610	10 below 65 "
Hanover.....	373	172	545	191	564	207	580	19 U. 35 "
W. R. Junction..	333	177	510	217	550	242	575	40 " 65 "
N. Hartland.....	323	177	500	227	550	242	565	50 " 65 "
Hartland.....	311	189	500	309	620	227	550	120 " 50 "
Windsor.....	304	196	500	236	540	216	520	40 " 20 "
N. Charlestown..	293	157	450	182	475	210	500	25 " 50 "
S. Charlestown..	284	166	450	216	500	206	490	50 " 40 "
Bellows Falls....	283-34	191-142	425	203-252	486	207	480	61 " 55 "
Walpole.....	226	169	395	239	465	224	450	70 " 55 "
Putney.....	215	135	350	150	365	210	425	15 " 75 "
Brattleboro.....	200	141	341	290	490	210	410	149 " 69 "
Northfield, Mass.	177	128	305	213	390	213	390	85 both

This table shows us first, that the terraces slope downwards at about the same angle with the river's bed. A lake or ocean is therefore not necessary for their formation, and for similar reasons the tributary deltas may represent floods greater than that of the main stream. A study of the strata of the deltas shows that they slope rapidly as if deposited upon a considerable pitch towards the main stream. Secondly, the tops of the highest normal terraces do not slope continuously and uniformly according to the figures in the second and third columns. This would seem to indicate a variation in the magnitude of the flood or of obstructions. It is most marked between Thetford and Wells river, where the height is less than half that of the same terrace above and below. It is possible that the greater altitudes of the terraces below Thetford may have been occasioned by an inequality in the bottom of the

river. The kame gravel beginning at Thetford may have constituted an obstruction to the free flow of the river. The gradual rise northwards of the deltas in column seven does not agree with the inequalities of the same as stated in column five. Thirdly, the last column shows the differences in the height of the deltas and normal terraces given by both authors. If we are not required to accept the deltas as a measuring rod, we shall largely reduce the depth of the stream and thus learn why the velocity as calculated by Dana is far too great. If the value of a theory consists in its ability to explain phenomena, then certainly the view that the lateral normal terraces indicate the height of the glacial flood is to be preferred to that which would make the tributary deltas the index.

The other point of difference between these authors concerns the existence of a kame between Thetford and Windsor. The one describes an arched ridge of coarse gravel and sand following the lowest level of the valley and dipping beneath all the terraces. The other insists that this gravel is a series of beds capping the higher terraces.

The best locality to settle this question lies about a quarter of a mile from my residence. I have passed it hundreds of times, and always observe the relation of the two deposits in passing, and remark how the kame-gravel dips beneath the terrace loam. I have examined this and several other similar sections described by Professor Dana — as upon both sides of White river — and with this gentleman's printed paper in hand, so as not to misunderstand his views, and find that Mr. Upham's sections and descriptions are entirely correct.

Dana accounts for the presence of the very coarse gravel in the middle of the valley, by supposing it to have been brought down by tributaries. Thus the coarse gravels at White River Junction are thought to have been brought down White river and dropped where the tributary joined the Connecticut. If so, then the gravel should be composed of pebbles corresponding in character with the rocks in Vermont; but the most of them consist of material derived from the north. As the glacial current came from the northwest, these fragments of northerly origin could not be distributed much to the west of the Connecticut. Scarcely any are to be found west of the kame, thus proving the gravel to have been deposited by the Connecticut flood and not by the White

river. Furthermore, this tributary enters the main stream through a gorge-like channel pointing south of east. Its sediment could not be distributed to the north up and against the Connecticut current, but this gravel bed is continuous for *one-and-a-half miles up the Connecticut* above the mouth of White river. Hence it could not have been brought by the White river.

Accepting, therefore, the existing of a kame in the Connecticut, we have still further reasons for reducing the size of the glacial flood, and thus of harmonizing our theories with observed facts.

Mr. Upham's careful studies of the modified drift of the Merrimack and the other New Hampshire rivers do not indicate such differences between the normal and delta terraces as have been pointed out in the Connecticut. Calculations of the volume and velocities of the glacial Merrimack, Contoocook and Saco rivers would be based upon the altitudes of their highest normal lateral terraces.

CONTRIBUTION TO SEISMOLOGY. By RICHARD OWEN, of New Harmony, Indiana.

[ABSTRACT.]

THE object of the paper is to point out the relation between solar and terrestrial dynamics, especially during the shorter secular changes: to show that when there is even a slight increase or diminution of motive power in the sun, there is, both as regards time and space, either directly or indirectly, a corresponding increase or diminution of the earth's kinetic energy, bearing on inorganic and also on organic matter.

DEFINITIONS, EXPLANATIONS AND ABBREVIATIONS.

The words force, power, energy are used as synonyms, denoting the inherent attribute of bodies, which gives rise to the various forms of motion, whether molecular or mass. The adjectives motive, kinetic, dynamic are also used as interchangeable, without essentially varying the meaning. "Seismic" is employed in

its most extended sense; embracing the phenomena of earthquakes, volcanic outbursts — whatever the material erupted — geysers, hot springs, gaseous emanations from solfataras and fissures, as well as land elevations and depressions. Great circles are abbreviated to G. C., and are considered as belts having a surface width about fifty miles each side of the theoretical line. The five equidistant G. C.'s described, in the Boston paper[1] for American Association for the Advancement of Science, as east continental trends, and which are secondaries to the ecliptic at our summer solstice, will be designated, beginning at Macassar's Straits, as A, B, C, D, E; while their mates, defining chiefly western trends, and being secondaries to the ecliptic at our winter solstice, will be called A′, B′, C′, D′, E′. The two terminators for noon and midnight at the meridian of land centre, described on p. 445 of said paper, are abbreviated as F and F′, while the meridian of Mount Rosa (land centre, in about 8° E. long.) may be called P. M., and its prime vertical, whose seismic importance will be shown, is designated as P. V.

In an abstract, necessarily, only a few illustrative examples can be given, thereby detracting much from the materials for a general deduction, and lessening the probability of its acceptance.

I. *Distribution in space of kinetic energy, whether molecular or mass-motive.*—Let us trace (best with charcoal) on any Mercator's map, which exhibits the volcanoes and earthquake regions, all the G. C.'s given in the above paper on "Land-forming," as well as the P. M. and P. V. To these add the concentric circles for each continent, as described in same paper, as well as for Mt. Rosa, the centre of the land hemisphere.

Additional, and not of least import, are the radial fissures (resulting probably from cooling and contraction) diverging from these land centres. The most important usually form with each other, angles[2] at the focal centre of $\frac{3}{3}\frac{6}{0}° = 120°$; while others are $\frac{3}{6}\frac{6}{0}° = 60°$, or sometimes even $\frac{3}{12}\frac{6}{0}° = 30°$.

Having traced these enumerated circles and fissures, we shall find that we have now covered, with our charcoal belts, almost

[1] Boston Proc. A. A. A. S., pp. 437-446.

[2] The discussion of the above law being found too extensive, as at first designed, for a foot-note, and being considered to apply with equal force in plant development, is made the subject of a short, separate paper.

every portion marked in the maps as seismic, except Jan Mayen in the north, and Mt. Erebus in the south. Furthermore the paper shows that the G. C.'s follow the mountain ranges and the junctions between geological formations, besides several other points, in illustration of which a few examples are subjoined under their appropriate headings.

1. *These great circles accord with mountain ranges.*—Witness the Appalachians and western Ghauts parallel to D; the Rocky Mountains, Great Khingin and Stanovoi to F; the Sierra Nevada to D′; the mountains of Greece, Turkey in Europe, Bohemia, etc., to B′.

2. *These G. C.'s mark geological trends between two formations.* D passes along eruptive rocks, separating Palæozoic from later formations in the United States; B′ in Greece and Turkey runs between Cretaceous and Tertiary, in Bohemia between Palæozoic and newer rocks; then again going N. W. passes between Mesozoic and Cenozoic.

3. *The intersections of G. C.'s are highly seismic.*—Santorin is at the intersection of B′ and E′; Caracas at that of A and P. V.; Sumatra and Quito at the junction of three G. C.'s; Iceland of two; San Salvador of two; Lisbon of two; the extinct Eifel volcanoes of three, viz., P. M., B′ and C; Japan, at its V. Fusi-yama, of two; the Cutch, with its Ullah Bund, of two, viz., D and P. V.; the submarine Atlantic volcanic region of two, C and C′.

4. *They mark linear volcanoes, rising and sinking areas, also geysers, hot springs, etc.*—The volcanoes of Kamtchatka, the Kuriles, Japan, etc., are on A; and those of South and North America chiefly on F. The Greenland subsidence is on B′ and E; east end of Candia sinking is on B′; the west end on E′ is rising. The area in north of Scandinavia, now rising, is traversed by D, F′ and A′; the south portion, sinking, is crossed by C. The great subsidence in the Pacific seems mainly due to E and E′; also to C and C′; while lake depression of central North America[3] is due especially to A. The oscillations in Belgium,[4] as well as those in the Danish Island of Möen[5] are on belt C. The geysers of Iceland are found on D and B′; those of our "National Park" on B′; the fires of

[3] Dana's Man. of Geol., p. 391.
[4] Encyclop. Brit. (9th Edit.), Vol. III, p. 444.
[5] Lyell's Antiq., p. 393.

Baku are near P. V.; the gaseous emanations (now extinct) of Delphi and Livadia on B′; the dog-grotto carbonic di-oxide, near Naples, is on the fissure from Rosa, through Stromboli to Messina.

5. *They connect places of synchronous, earthquake tremblings.*—Quito and Ochotzk[6] are on F; Malta and Scotland on the direct fissure connecting those two places through Rosa; Milan and Tornea on fissure from Rosa to Baltic; Messina and Upsala connect at Rosa by two fissures forming an angle of 120°; yet perturbations at Berlin did not extend to Paris[7]. Scandinavia is connected with Iceland by D, but not with Great Britain.[8]

6. *The areas of these seismic belts seem favorable to metamorphosis and crystallization of rocks and minerals.*—In the Grecian Archipelago, along B′ and E′, the limestones appear to have been all converted into marble — it is so abundant that even the curb-stones at Athens are marble — and along D we observe similar effects in North Carolina, Virginia, Maryland, New York, and Vermont; and the radial line of force from Rosa, skirting between the west coast of Italy and the left flank of the Appenines, through erupted rocks, aided to give Carrara and other cities, near that line, their marble, they being on the radial belt from land centre to Calabria, Messina and Etna. The belt A passes through the diamond region of Brazil; E runs close to the African diamond district, while D is near the Golconda and Ceylon areas, noted for gems.

7. *The magnetic force exhibited in the earth's crust to the east of Kew and Lisbon*, north of Vienna and northwest of Petersburg, described[9] by Prof. Grylls Adams, is fully explained by following great circle C; also *Low Barometer*, possibly due to tension *prevailing sometimes along belts of G. C.'s, seems to predispose those areas to linear storms.* No. VII, of the Signal Office records,[10] followed the track of D; the hurricanes of our coast, originating near Florida, run parallel to D; and the typhoons of Japanese waters are parallel to F and A.

[6] Mallet's Cat. of Earthq., 5th Rept., pp. 55-6; also Plate X *bis*.
[7] Cosmos, Vol. IV, p. 143.
[8] Mallet's Cat. of Earthq., 4th Rept., p. 3.
[9] Lecture delivered at Royal Institution, June 3, 1881 (Nature).
[10] Houston's Phys. Geog., p. 140.

8. *At least three of the foci of magnetic intensity are at the junction of two G. C's.*—The sub-Australian focus is at the intersection of D with E; that of Siberia is near where B and E′ meet; that in the South Pacific is at the junction of A′ with E′.

9. *These belts traverse chiefly fertile tracts of land, and often pass through regions of extinct gigantic animals.*—The densest forests of South and North America are on A and D; while the giant Sequoias of California are on D′, not far from its junction with B′. The extensive forests of Bohemia, Thuringia, etc., are also on B′. Belt D′ covers the Patagonian area of Glyptodon, etc.; P. V. and E that of the New Zealand Dimornis, etc., A′ and C′ of the Australian Diprotodon and the like; A and D and B′ of the gigantic remains in the United States; B and E′ those of N. Siberia; F′ those of the sub-Himalayas.

10. *The chief civilization appears often to follow continental trends marked by G. C's.; and in Europe also the P. M.*—The civilization of eastern Asia is covered by A and F; that of Africa mainly by B and B′; in South America F marks the older civilization of the Incas, the Yucatan architecture, then passes near the Aztec, Toltec, and perhaps Arizonian developments; while the mound-builders spread along A, and later Anglo-Americans along D; latest on belt D′. In Europe the palæolithic, and neolithic evidences in Scandinavia and Denmark, the *find* at Neanderthal, the lake dwellings in Switzerland, and the remains in the caves of Engis, Mentone, etc., are all on or near the P. M.

11. *National and individual development seem greatest on some of these belts.*—Grecian and Macedonian history followed B′ into Austria and Germany; Rome spread her power along the fissure which traverses Sicily, Rome and Rosa (Helvetia), through Gaul to Britain; the civilization of Spain, France, Belgium, Holland, Prussia and Denmark is found on C; while F′ traverses Scandinavia and western Russia to near Gangetic Hindostan (Benares, etc.), as does the P. V., the Caucasus and Persia.

Should this law be found to prevail, as it has thus far, in numerous individualized applications, it would point to an additional important factor in the influences of environment; and when we find the belt of B′ passing through *eastern* Greece, the law may aid us to understand (especially in connection with the distribution

of kinetic energy in *time*) why the inhabitants of Attica were the ablest of the Greek sub-races, and why, between the years 530 and 430 B. C., Attica should have given to the world fourteen of the most illustrious men in history out of about 135,000 males, counting three generations to a century.[11]

II. DISTRIBUTION OF SEISMIC PHENOMENA AS REGARDS TIME.

The tabular synopsis, exhibited with the unabbreviated paper, shows that seismic phenomena group themselves around the sun-spot periods; for instance, Etna was quiescent from 1694 to 1755; the years 1698 and 1753 being two of the consecutive fifty-five year minimum periods. The synopsis further demonstrates that, by computations from Mallet's "Catalogue," earthquakes were much more numerous, relatively to the whole number for a given period, in middle Europe about the year 1663 (when there was no declination of the needle in middle Europe), than for the same number of years before and after 1814 (the period of greatest westing of the needle in middle Europe), apparently indicating that maximum seismic activity gradually follows the changes in the magnetic meridian of a continent.

The tabular view shows also that sun-spot periods coincide with meteorological anomalies (such as a severe winter followed or preceded by a hot summer). The geothermal changes, in middle latitudes, perhaps following the atmospheric maxima and minima after six months,[12] may, by differentiated heat (as when at the heated junction of the Bismuth-Copper Thermo-multiplier, the Bi., unable to transmit heat as rapidly as Cu., converts a portion into electricity), produce magnetic anomalies in those years of thermal anomaly, as indicated by the magnetic needle standing still, or even retrograding for a year or more, instead of pursuing its regular secular march.

Although the whole magnetic system is undergoing secular changes, the mean plane of oscillation seems to coincide with the meridian of land-centre, and to pass through the terrestrial equator between St. Thomas Island and the coast of Guinea. There

[11] Lyell's Antiquities, p. 547.
[12] Cosmos (Sabine's Trans.), Vol. IV, p. 38.

the equator of dip intersects the equator of rotation, and the equator of intensity crosses this same meridian of land-centre farther south about 10°–15°; both at present running somewhat parallel to our winter ecliptic, while in perhaps three hundred and thirty years (as estimated below) these equators will run nearly parallel to that of our summer solstice. Additional evidence of the land-centre meridian being also the mean central plane for the magnetic system is found in the fact that (on using this meridian as the dividing plane), the sum of the magnetic intensities in the eastern hemisphere is almost exactly equal to the sum of the intensities in the western;[13] also that the measurements from the region of least magnetic intensity south of St. Thomas to at least three of the foci of maximum intensity, as well as to Boothia Felix, are as nearly equal as are their relative intensities. Again the equator of dip near St. Thomas does not differ in position much from the mean equator of heat (mean isothermal of 80° F.) and the northern foci of greatest intensity do not vary far from being the poles of maximum cold in North America and Siberia. The close conformity of isocheimal lines to isoclinics is also pointed out, and some explanation is offered why in our Middle States and Alaska the declination E is diminishing, while in California and some adjoining regions it is increasing.

Eminent astronomical authority admits not only the above discussed variation in the sun's activity, but also its bearing on the earth, thus:[14] 1. "The equatorial regions of the sun have at least $\frac{1}{16}$ higher temperature than the portion beyond the thirtieth parallel of lat. 2. The temperature of the sun is a trifle higher in the northern than in the southern hemisphere. 3. The sun-spots emit less heat and light than the other regions." Another astronomer[15] says: "In 1637 sun-spots were so numerous that the heat and brilliancy of the sun were perceptibly diminished;" and again:[16] "Sun spot periods coincide . . . with variations of magnetic force and the periodicity of auroræ boreales. The periodicity of the spots indicates, as Father Secchi remarks, a periodicity in the solar activity . . communicated to the earth."

[13] Polyt. Review for Dec. 14, 1878, p. 247.
[14] Father Secchi on the sun, p. 133.
[15] Rambosson's Astronomy, p. 70.
[16] *Ibid*, p. 75.

Any gradual change in the sun's power, which would modify the thermo-electrical influence, might directly or indirectly produce the secular change in the needle from greatest easting to greatest westing and *vice versa*. As to the time in which this takes place, although we have not yet sufficient data for any certain calculations,[17] some coincidences seem to point to a period of about three hundred and thirty years, thus:

30 ×	11, the minor sun-spot period	= 330 years.
6 ×	55, " major " "	= 330 "
2 ×	164, twice the period of Neptune	= 328 "
4 ×	84, four times the period of Uranus	= 336 "

Possibly these combined influences may have some bearing in connection with other forces (perihelion, perigee, etc.) in effecting an electrical and magnetic predominance, thus, as remarked, producing greatest westing when the maximum force prevails in our winter solstice, and greatest easting when the most energy exists in our summer solstice.

By tentative calculation, taking the ratio of the secular period at Paris from 1663 to 1814 (the arc of variation from Boothia Felix to N. Siberia forming at St. Thomas Island an angle of about 23°–24°, and at Rosa fully double that arc, while at Paris it is greater, but unequal for easting and westing), the secular period of magnetic oscillation for middle Europe might be about as above, nearly . . = 330 years.

An evident relation is also traced and exhibited in the synopsis between those centuries marked by Mallet as seismic (first, fifth, ninth, twelfth, and eighteenth), showing them also to be historically of great importance; while, on the contrary, few stirring events occurred in those centuries recorded by Mallet as non-seismic, viz., the first and second B. C., as well as the third, seventh, tenth and fourteenth centuries of our era.

This suggestive thought is worthy of further investigation, in order to trace the influence of environment on anthropographic and ethnographic development.

[17] Report of 1881 by Chas. A. Schott, Esq., Assistant U. S. Coast Survey.

Law of Fracture, or Fissuring, applied to Inorganic and Organic Matter. By Richard Owen, of New Harmony, Indiana.

[ABSTRACT.]

As the unabbreviated paper applies the law to inorganic and also to organic bodies, this abstract commences with the former.

I. *Inorganic.* If pellets of clay, or other plastic spheroids, be dropped into a bowl, the lateral pressure compels them to assume, and present to the eye, an approximately hexagonal form.[1] This is the polygon of greatest sides, susceptible of lateral junction with similar polygons, so as to leave no intervening spaces. We find, on examining this junction of spheroidal bodies, compressed while plastic, that the lines of contact present, where any three of these polygons come together, three fissures or lines of fracture, diverging at angles of $\frac{2}{3}60° = 120°$. If, however, these hexagonal fissures again break up into equilateral triangles, by the prolongation of these sutural chord fissures into the adjoining hexagons, then these six prolonged fissures meet at a central focus.

Further, if we break a pane of glass, or a cake of cap cement, by a smart blow of a sharp instrument, placed centrally on the pane or cake, we find that the radiating cracks or fissures, thereby produced, present angular fragments having, at the focal point a measure of 60° or 30° or occasionally of 120°. This law can also be traced in the cooling and shrinking of basalt: thus giving rise to a set of vertical fissures diverging 120°, as in the separation between the columns of the Giant's Causeway. The same tendency can be traced in mud when drying, although often sun and wind, acting chiefly in two directions, may give rise to right square prisms. Fissures resulting from an upward force have radial earth fractures, as after the earthquake in Calabria.[2] If the columns separate also horizontally, then we have such exhibitions as at Bertrich Baden Cheese Grotto.[3] Should the superincumbent layers be so distributed, as in bee cells, that the unit of an upper layer is directly over the triangular fracture between three contiguous units below, and in its turn be pressed upon by three units above, each such unit would in the upper layers be enclosed by nine rhombic planes, and in the intermediate layers would assume the dodecahedral form.

[1] Key to Geology of the Globe, p. 85.
[2] Lyell's Prin., II Vol. p. 124; also pp. 127 and 128.
[3] Lyell's Elem. (ed. 1856) p. 485.

In the separation, however, of contracting bodies, as well as in the compression of adjacent plastic spheroidal bodies, if some cells or units are smaller than others, or if the pressure is unequal, or the ingredients not homogeneous, the result may present a pentagonal cross-section. The hexagonal and radial system of fissuring has apparently nothing in common with the so-called joints, well described by Prof. G. K. Gilbert,[4] which are often at right angles to the planes of stratification, and parallel to each other; nor with slaty cleavage, probably due to such compression as forces all the planes of the fragments (*e. g.*, plates of mica in mica slate) into the same plane with the compressing force.

Although, on our globe, the five equidistant great circles of least resistance (or greater seismic force from interior) and their mates, in consequence of being secondaries to the ecliptic at the solstices, would point to solar influence as at least contributing to give them importance, yet the tendency of the solid materials in the earth's crust, particularly at continental centres, to separate by definite radial fractures, when acted on by erupting force from beneath, exerted at various geological periods, evidently follows the same law of fissuring stated above: the fissural lines being sometimes marked by a chain of lakes (as from the centre of North America running N. N. W.); or an extensive gulf (as that of Guinea or Genoa); or occasionally by interrupted elevations, islands and mountains, such as the line from the centre of Africa, near St. Thomas, running N. N. E. through Prince's Island, Fernando Po to the Cameroons and other mountains, as well as S. S. W. to St. Helena; or the 120° divergence in the Carpathians from the Tatra centre of Europe; or the Altai range running S. W. from the centre of Asia, while another range, diverging from it 120°, trends S. E. toward Lake Baikal. By bringing Mt. Rosa to the zenith, we can readily, on extending a string thence successively to every 30° around the globe, trace twelve radii, marking usually depressions, some radii more important than others because passing through highly seismic regions, thus:

1. Calling the meridian of Rosa which runs close to Corsica, Sardinia and Tunis, the Prime Meridian (although Greenwich Meridian is eight degrees W.), we have the termination in the reentering angle at the Gulf of Guinea, the true centre of Africa. 2. Diverging thirty degrees E. we have a highly seismic belt

[4] Am. Jour. of Sc., July, 1882, p. 50.

from Rosa, covering Elba, Rome, Naples and Etna; broad enough to take in also Calabria and Malta, thence to lakes Albert and Victoria Nyanzi, and the west coast of Madagascar. 3. A seismic belt, marking the Adriatic and Santorin, passing between the Dead Sea and Mt. Sinai to Cape Gardafui. 4. The E. extension of the Prime Vertical, through the Black Sea, the S. shore of the Caspian, and the Demavend Volcano, to Cutch and the reëntering angle N. of Madras. 5. The line through Tatra, Europe's centre, to the base of the Urals. 6. Through the Baltic to the White Sea. 7. The N. end of our Prime Meridian running through Behring's Straits, etc. 8. A belt, through the E. coast of England and W. of Scotland to Iceland and Greenland thence through Hudson's Bay to Vancouver's reëntering angle. 9. A belt through Mt. Blanc to the Land's End, England, through the Bay of Fundy and along the Appalachians to the Gulf of Mexico and the Volcano of Colima. 10. The W. end of the Prime Vertical, along the Cantabrian Mountains, through the Azores to Caracas, Quito, etc. 11. A S. W. belt through Cape Vincent, probably between Madeira and Funchal to near the Canary Islands, and through Brazil in a line parallel to the mean trend of its mountain ranges, finally to the disturbed region of Valparaiso. 12. A S. S. W. belt through Minorca, Liberia and Ascension, to the Falkland Islands.

Pieces of pasteboard, cut with angles of 120°, 60° and 30° will be found convenient for measurements, when applied to globe, map or organism. The five great circles, which in the paper on land forming were shown to be 72° apart at the equator, form angles of 60° at their intersection near the polar circles, and enclose successively: 1, Asia; 2, Europe with Africa; 3, S. America; 4, N. America; 5, Oceanica and Australasia.

The above law of fracture seems also to apply to the confluence of rivers: primary tributaries usually forming, with the great continental drainage stream, angles of 60° (as the Missouri and Mississippi) or even 120° when belonging to separate drainage systems (as the Ohio and Mississippi rivers); while the secondary confluents anastomose sometimes at angles of 60° (as Wabash and Ohio); but occasionally at 30°; third-rate tributaries usually enter those of secondary importance at angles of about 30°, as White river at its junction with the Wabash, or Washita with Red River.

The paper next passes to an examination, how far the law applies to plants:

II. *Plants.* If the primordial cell, with its protoplasm and central or subcentral nucleus be in juxtaposition with similar cells, and somewhat crowded, each cell assumes a more or less hexagonal form;[5] and a unicellular plant, such as protococcus, offering an epitome of vegetable life, presents, when young, the division into six equal triangles, whose sides radiate to the central nucleus.[6] In all cellular tissue,[7] as well as in vascular tissue, we can trace more or less of the hexagonal cross-section, and even the tendency to radial fissuring. From this cause, apparently, in the verticillate forms of plant growth, such as that of the pines, we find sometimes three branches thrown out horizontally, each at an angle of 120° from the other, oftener six at equidistant angles of 60°. Whereas, in most of those plants having a spirally ascending growth, we find branches, or petioles, given off, whether opposite or alternate, at angles which form an aperture of 120° with the lower part of the trunk or rachis, and angles of 60° with the ascending stem, and which in the alder cycle (fourth leaf above first) diverge from each other at angles of 120°, when viewed in horizontal section. The axillary bud, whether leafy or floral, emerges from the stem at an angle of 30°. When the hexagonal type prevails, as in many monocotyledonous plants, it seems natural that at those six angles, where the least resistance is offered to dehiscence (the most pressure being near the centre of each hexagonal line of cell-junction) the greatest facility should occur for gemmation; the same applies to the pentagonal arrangement, so prevalent among exogens; also to the four-parted or cruciform type; from whatever cause these modifications may arise. The angular divergence indicated above can, by means of the pasteboard angles, be very readily measured in young timber or among the commonest flowers and weeds: the 30° growth probably predominating in excurrent trees, while the 60° branching is characteristic of the trees with solvent axis; and is found in the great majority of common plants. The divergence of secondary branches, at an angle of 60° from the primary, brings the former approximately parallel to the ascending axis, thus lessening the tendency to breakage of tree limbs, while presenting leaf surface well distributed to air and sunlight.

The coalescence of adjacent side walls of cellular tissue, running as medullary rays, from the central pith to the epidermal layer of

[5] Ency. Brit. (9th edit.) p. 73. [6] Ibid, p. 81. [7] Gray's Les. in Bot., p. 145.

the plant, gives rise, by the union of two triangles of cellulose-enclosing parenchyma, to a rhombus, which by lateral pressure and upward growth becomes very acute, and presents, in the bark, those diamond shaped areas found on hickory, black locust, and the like, while the bark markings on some trees of less rapid growth, approach, in their obtuse rhombic form, almost to squares. The crowding of each separate seed against its neighbors, in the large cone, borne by the Italian pine, exemplifies also the hexagonal system; and the tricarpellary dehiscence of the seed vessel in Köhlreuteria (a Maplewort) with its three midvein dissepiments, exhibits three equal compartments for the seeds, which in other pericarps, by duplication, may become hexamerous.

III. Animals. Among the Actiniæ,[8] we find the hexameral arrangement of primary mesenteries, after the normal type; their six fleshy septa extending from the outer edges of the column wall to the centre, while the secondary or intermediate mesenteries do not reach the centre. Among corals the disposition of the six primary septa, and additional forty-two intermediate septa, especially of the corals usually assigned to the order *Zoantharia sclerodermata*[9] (as well as the crowded corals in some specimens of the extinct *Cyathophyllum rugosum* is strikingly conformable to the hexagonal type, with strongest radii to points of least resistance. The gemmation among some of the Radiates, as when "the Hydroid Zoophytes develop, at a diverticular prolongation of the soft interior substance of the stem,"[10] a Medusa bud, closely resembles plant-budding; and the bud prolongation diverges at an angular aperture of either 60° or 30°.

Among Articulates the *Coronula diadema* is a symmetrical six-sided prism, divided into six triangles not quite equal. The same beautiful provision for strength and susceptibility of contraction and expansion can be traced in the scoriaceous integument of the trunk fish (*Lectophrys camelinus*); and even, although not with such perfect regularity, in the attenuated snout of the paddle fishes (Spatularia).

The law is wonderfully illustrated in spiral univalve shells; depressed spires, as those of Solarium, Patella, Natica and Pyrula, showing 120°; medium spires, as those of Strombus, Ranella and Struthiolaria, forming an angle of 60°; more elongated spires, like

[8] Ency. Brit., Vol. vi, p. 328. [9] Ibid, Vol. iv, p. 382.
[10] Carp. Prin. Comp. Phys., pp. 544-6.

those of Cerethium, Mitra, Terebra and Melania, diverging 30°. Even the nodes, on the spirally ascending whorls (showing greater power or facility of development where they occur) when we place the vertex of the 30° pasteboard angle at the spire-apex and extend the sides to the nodes, evidently form with the axis of the columella usually angles of 30°. In some large Pyrulas, however, on the outer whorl, we find at 30° only an almost obliterated nodule; but the 60° aperture reaches a prominent node.

Oken recognized the vertebral bone as the type of the skeleton: and in the neural arch, as well as the hæmal,[11] we find six bones, while the atlas and pelvis have an evident tendency to the hexagonal form; and in the enamel of the teeth is shown, in the beautifully regular columns with six sides, an arrangement giving great strength for resistance to crushing force. The cell, however differentiated (*e. g.*, in mucous membrane[12]), is still the type of all organisms, and retains more or less of the hexagonal form.

In the animal, as in the plant, the five-parted form, here chiefly designed for prehension and locomotion, often prevails. Thus among radiates, the crinoids partake largely, both in body and extremities, of the pentagonal type, although some of the Cystidea (as Marsupites, Cariocrinus, etc.) are symmetrically hexagonal, even in detail. Of the Mollusks, the highest Dibranchiates exhibit the five tentacles duplicated. Among Articulates, the true insects conform to the hexagonal; but the Decapod Crustaceans exhibit the twice, or bilateral, five-parted type. Vertebrates give us often, among teleost fishes, five finny appendages (two pectoral, two abdominal, one dorsal, and sometimes, however, also one anal, to form the sixth). The highest reptiles (Crocodiles and Alligators) have five toes; among birds, the spur of the Gallinaceæ, added to the four toes, may point to a rudimentary fifth; the highest orders of Mammals, the Carnivora, Monkeys, and Man himself, all conform to the five-parted type of *ultimate* extremities. Indeed, if we include the neck with its sometimes elongated jaws, and the tail often prehensile, as both belonging to the *proximate* appendages, then we have in them also those two prevailing types, 5 and 6.

Again, as in the confluence of rivers, the paper shows the anastomosing of arteries, veins, and even nerves, as very generally

[11] Owen on the Skeleton and Teeth, pp. 27 and 28.
[12] Carpenter's Prin. of Comp. Phys., p. 406.

according with the law of 60° aperture, for important branches, and 30° for those of secondary importance. Occasionally the angle is one of 120°, where the veins, etc., come together from nearly opposite directions.

SUMMARY.

The law that homogeneous *inorganic* matter often fissures at angles of 120° seems derived from the fact that, when plastic clay-balls, or other spheroidal masses are acted upon by equal force from all directions inward, they are compressed into hexagonal prisms: each contiguous three separated by angles of 120°; and from the additional fact that again, when homogeneous matter is compelled to separate, by disruptive force acting equally in all directions outward, it fissures into hexagonal prisms. By the reciprocal prolongation (into adjacent cells) of the three fissures described above, each hexagon is liable to be separated by fissures (or in some organisms by partitions) into six equal triangles. In the case of Basalt, any three hexagonal columns, resulting from disruption, would offer at their focal junction, apertures for subsequent outflows: thus adding new matter to the external earth's crust. Such fractures, from whatever cause, may occur at different periods of the earth's history, often giving rise to "faults" of hundreds and even thousands of feet;[13] and may then close either permanently or temporarily. This applies especially to the opening of cracks from below:[14] such as those in Iceland, caused by an earthquake,[15] and reported to be forty miles long. The five equidistant great circles, on our globe (described in another paper), supposed to be belts of least resistance, probably owe their origin, at least in part, to external forces, inasmuch as they are secondaries to the solstitial ecliptic; some are also terminators for important continents during the sun's meridian fervor.

Plants exhibit the same hexagonal tendency in their cells, etc.; and send out their buds at symmetrical apertures of medullary tissue in the stem or branch:[16] the constriction at each end or joint apparently giving greater activity to the side buds.

[13] Fisher's Phys. of Earth's Crust, p. 213.
[14] Ibid, Phys. of Earth's Crust, p. 201.
[15] Ibid, Phys. of Earth's Crust, p. 189.
[16] Carpenter's Comp. Phys., p. 519.

MAP OF LURAY CAVERN.

1. The Vestibule.—2. Washington's Pillar.—3. The Flower Garden.—4. The Amphitheater.—5. Natural Bridge over Muddy Lake. 6. The Fish Market.—7. The Crystal Spring.—8. Proserpine's Pillar. 9. The Spectral Column.—10. Hovey's Balcony and Scarfs.—11. Oberon's Grotto. 12. Titania's Vail. 13. Saracen's Tent, and Fallen Column.—14. The Organ and Throne.—15. The Tower of Babel.—16. The Empress Column.—17. The Hollow Column.—18. Henry Baird (or Double) Column.—19. Chalcedony Cascade.—20. The Coral Spring.—21. The Dragon of Luray.—22. Bootjack Alley.—23. The Mermaid, or Scaly Column.—24. The Lost Blanket.—25. Helen's Scarf.—26. Chapman's Lake.—27. Broaddus Lake.—28. The Castles on the Rhine.—29. The Imperial Spring.—30. The Skeleton.—31. The Twin Lakes.—33. The Engine Room.—33. Miller's Room.—34. Hawes' Cabinet.—35. Specimen Avenue.—36. Proposed Exit Avenue.

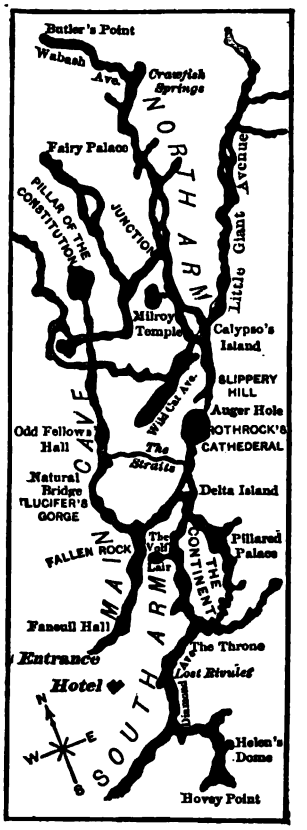
NOTE. In connection with the above paper, maps were exhibited by the author, showing the results accomplished in surveying Mammoth, Wyandot and Luray caverns, copies of which are here reproduced. At the evening reception given by President Dawson at the Redpath Museum, Mr. Hovey gave illustrations of cave scenery, by means of a lantern, from recent photographs taken by means of electric light.

SUBTERRANEAN MAP-MAKING. By HORACE C. HOVEY, of New Haven, Conn.

[ABSTRACT.]

THE difficulties of subterranean map-making are peculiar. The task is very different from that of surveying a bit of meadow or prairie. Were it the more complicated one of mapping a region diversified by hills, lakes and rocky glens, yet there would be the advantage of doing the work by daylight. But an underground survey must be managed while creeping under low arches, following rugged and tortuous paths, skirting the edge of deep pits, fording miry pools, or climbing over rocky fragments piled under lofty domes. And all this diversified realm is wrapped in absolute darkness, but imperfectly scattered by lamps. Imagine a map of the Adirondacks made by torchlight on a series of moonless nights, and it will give some conception of the hindrances to a complete survey of such a place as Mammoth Cave.

MAP OF WYANDOT CAVE.



Add to this the fact that the singular barometric conditions make tests of altitude doubtful; and that the compass is often affected by magnetic disturbances. Consider, also, the natural reluctance of cave owners to have measurements taken that may show the limits of their surface rights to be less than their subterranean claim. No wonder that such a formidable array of obstacles should long have hindered the attainment of even approximately satisfactory results.

In exhibiting, therefore, maps that have been made of three of the most noted American caverns, namely, Mammoth, Wyandot and Luray, it should be understood that little more is claimed for

them than that they are helpful diagrams, and that they are the best results thus far secured from subterranean map-making in this country.

1. *Wyandot Cave*, in Crawford county, Indiana, was purchased by H. P. Rothrock from the U. S. government in 1820; but no attempt was made to survey its avenues till 1851, when the bearings and distances were taken by H. C. Grosvenor. Another survey was made in 1853, by D. L. Talbot, M. D. From these materials, supplemented by further explorations in 1854, I prepared the first complete map of this cave, the original draught of which is still in my possession. Mr. G. I. Langsdale afterward laid down some later discoveries; and the map, thus jointly constructed, was published, in 1860, in Owen's "Geological Reconnoissance of Indiana." My work was carefully revised by myself, in 1878, and published. About the same time Prof. J. Collet made a new map, from notes of his own, which appeared in the Rep. Geol. Sur. Ind., 1878. While differing from each other in minor points, the main features of these two maps agree, thus proving their general accuracy.

2. *Mammoth Cave*, discovered in 1809, is located in Edmondson county, in Kentucky, 85 miles S. S. W. of Louisville. It has been repeatedly mapped, but with remarkable variations. The first rude outline was prepared in 1814, probably by Mr. Gratz, the early owner of this cave, and published in the Med. Repository, vol. xvii, p. 391. It abounds in inaccuracies. A careful survey was made of the portion then known, in the winter of 1834–5, by Edmund F. Lee, C. E., who devoted three months to the task. His map, published by James and Gazley, of Cincinnati, O., is out of print and I have never seen any one who has examined a copy. Next came a map made from notes supplied by Stephen Bishop, the guide, and published by Morton and Griswold, of Louisville, Ky., along with a manual entitled "Rambles in Mammoth Cave." This, too, is out of print, the only copy extant being in my possession. It is said that the plate from which it was printed is destroyed. Dr. C. R. Blackall, of Philadelphia, made a map at a subsequent date, which, however, he was not permitted to publish, being restrained by an injunction. Another, but less accurate, map was prepared to go with Forwood's "Historical and Descriptive Narrative of Mammoth Cave." (Lippincott, 1870).

In the new map, prepared under my supervision, and completed

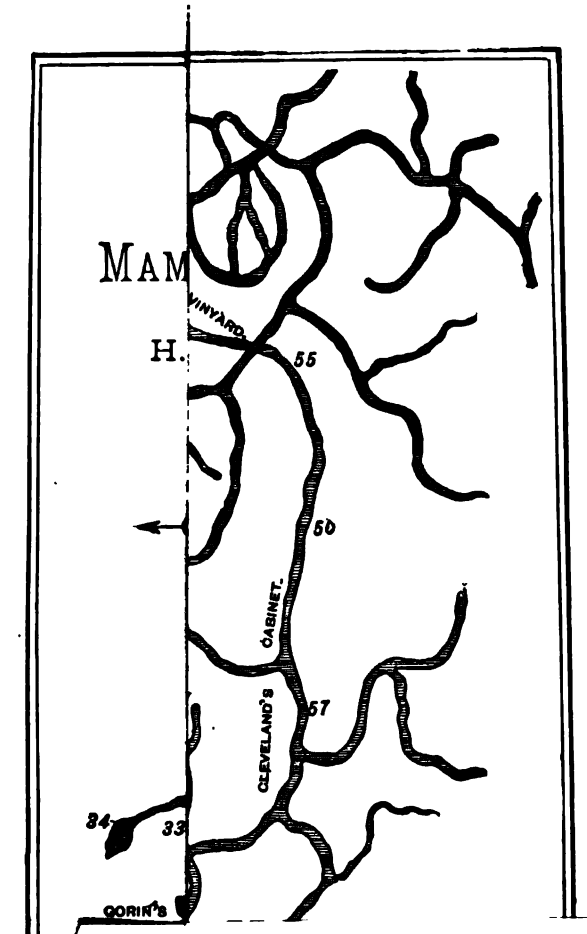
this year (1882), all the portion open to the public, as far in as the Echo river, is drawn after the recent survey made by Col. Francis Klett, manager of the cave, who had the advantage of previous experience in connection with the U. S. Geographical Survey. The portion beyond Echo river was drawn from a general survey of courses, the details being filled from older surveys, corrected by my repeated observations. I have not attempted to indicate more than 50 or 60 of the 223 avenues that are said to have been explored. The work is confessedly incomplete; but, such as it is, it comes nearer the truth than anything previously issued. The different levels are indicated by shading, the lighter shades denoting the higher galleries, and the darker marking the lower tiers and the pits and domes. The relative width of enlargements is only suggested as an aid to the imagination; the average being about 60 feet, and the greatest width not exceeding 400 feet. The vertical distance between the surface and the drainage level is about 328 feet. The general plan of the cavern is that of a vast labyrinth, with galleries crossing each other at various levels, the aggregate length of which is supposed to be 150 miles.

3. *Luray Cavern*, in Page county, Virginia, was discovered in 1878 by Mr. A. J. Campbell, and shortly afterwards was hastily mapped by Mr. A. Y. Lee, for the N. Y. Herald. Another map was made in 1880 by Mr. S. Z. Ammen. These were taken under the disadvantage of having to work by candle-light. This cave is now lighted by electric lamps throughout its entire extent, and by this aid a new survey was made this year for my use, with the endorsement from the manager, Mr. R. R. Corson, that "this is the only correct map of Luray Cavern ever made." The great room including the Elfin Ramble and Pluto's Chasm is about 500 feet long and 200 feet wide. Other large halls, *e. g.*, the Cathedral, and the Giants' Hall, are so filled with stalagmitic pillars as to prevent the visitor's getting a true conception of their vastness. Winding in and out among these mazes of stone forest and jungles, one is liable to magnify distances. The areal diameter of Luray Cavern does not exceed 2,000 feet, but the visitor usually imagines himself to have gone several miles under ground. The place has been aptly compared to the Centennial Buildings, when in their full glory at Philadelphia, amid whose embellished halls the visitor could ramble for days without satiety; and from which, on growing weary, he could at any moment withdraw, hoping to come again.

MAP OF LURAY CAVERN.

1. The Vestibule.—2. Washington's Pillar.—3. The Flower Garden.—4. The Amphitheater.—5. Natural Bridge over Muddy Lake. 6. The Fish Market.—7. The Crystal Spring.—8. Proserpine's Pillar. 9. The Spectral Column.—10. Hovey's Balcony and Scarfs.—11. Oberon's Grotto. 12. Titania's Vail. 13. Saracen's Tent, and Fallen Column.—14. The Organ and Throne.—15. The Tower of Babel.—16. The Empress Column.—17. The Hollow Column.—18. Henry Baird (or Double) Column.—19. Chalcedony Cascade.—20. The Coral Spring.—21. The Dragon of Luray.—22. Bootjack Alley.—23. The Mermaid, or Scaly Column.—24. The Lost Blanket.—25. Helen's Scarf.—26. Chapman's Lake.—27. Broaddus Lake.—28. The Castles on the Rhine.—29. The Imperial Spring.—30. The Skeleton.—31. The Twin Lakes.—32. The Engine Room.—33. Miller's Room.—34. Hawes' Cabinet.—35. Specimen Avenue.—36. Proposed Exit Avenue.

NOTE. In connection with the above paper, maps were exhibited by the author, showing the results accomplished in surveying Mammoth, Wyandot and Luray caverns, copies of which are here reproduced. At the evening reception given by President Dawson at the Redpath Museum, Mr. Hovey gave illustrations of cave scenery, by means of a lantern, from recent photographs taken by means of electric light.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Deep-sea Soundings and Temperatures in the Gulf Stream off the Atlantic Coast, taken under the direction of the U. S. Coast and Geodetic Survey. By J. R. Bartlett,[1] of Lonsdale, R. I.

[ABSTRACT.]

I have come to this meeting of the Association at the kind request of Mr. Hilgard, the Superintendent of the Coast and Geodetic Survey, to tell you of the investigations made by the steamer "Blake."

The hydrographic party of which I have charge has been investigating the Gulf Stream for the past four years, the last two summers having been passed on our immediate coast. All of you have ideas or pet theories in regard to this great river of the ocean, as Maury has happily called it. Many of our greatest scientists have written learnedly about it. Professor Bache, while he was Superintendent of the Coast Survey, caused extensive and systematic investigations to be made of its physical features. From the data obtained, the Stream was assumed to start from the Caribbean sea, making the circuit of the gulf of Mexico, and to issue with great velocity and high temperature from the straits of Florida, continuing to about latitude 45°N. It was described as a superficial stratum of warm water flowing over one of cold, with banks of cold water, and divided or bifurcated into warm and cold bands.

The bifurcation was explained by the presence of hills and valleys which were developed by the soundings, the cold water being said to be in the valleys and the warm water on the elevations. As no soundings were obtained in the strength of the current, it was supposed that the stream ran over, if not in, a great trough or depression of the sea-bed. At the time the investigations were made under the direction of Professor Bache, the means of obtaining the depth of the sea were very imperfect; now we have the most improved apparatus and soundings can be taken with wire in any current and to any depth with almost perfect accuracy.

The investigations made on board the "Blake" show conclusively that the above assumptions are wholly erroneous. My

[1] Commander U. S. Navy.

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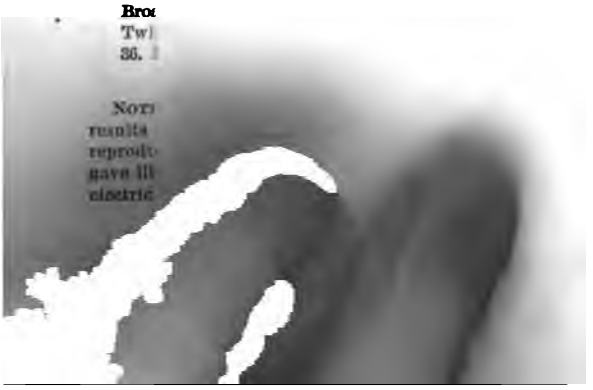

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[1] Commander U. S. Navy.

soundings give no channel for the Stream to run in, there are no hills or valleys, and I have never found any bifurcation or warm and cold bands. The temperatures obtained do not show cold water beneath the Stream, but a current of warm water between the Florida straits and cape Hatteras down to the very bottom.

As has already been stated to this Association by Mr. Hilgard, the current entering the gulf of Mexico from the Caribbean sea does not make the circuit of the gulf as formerly supposed, but passes to the northward and eastward in the same general trend as the Yucatan plateau, and thence to the straits of Florida.

The lines of soundings taken by the "Blake" were twelve in number, beginning at Jupiter inlet, Florida, and extending to Currituck, N. C. They were run normal to the general coast line.

The soundings give very interesting data in regard to the physical features of the bottom of the ocean over which the Gulf Stream flows. Instead of a deep channel in the course of the Stream, as reported by Lieutenants Maffitt and Craven, and published in the Coast Survey Reports, our later soundings with *wire* show an extensive and nearly level plateau extending from a point to the eastward of the Little Bahama banks to cape Hatteras. Off cape Canaveral this plateau is nearly 200 miles wide, and gradually contracts in width to the northward until reaching Hatteras, where the depth is more than 1,000 fathoms within 30 miles of shore. The plateau has a general depth of 400 fathoms, suddenly dropping off on its eastern edge to over 2,000 fathoms.

The course of the Gulf Stream can be traced by a study of the specimens of the bottom obtained; on each side of the Stream the sounding cylinder brought up ooze, but in the strength of the current the bottom was washed as bare of ooze and all living things as the bed of a mountain torrent. Instead of a cushion of cold water at the bottom, the temperatures were the same as those found at the corresponding depth of 400 fathoms in the Windward passage and in the course of the current through the Caribbean sea and gulf of Mexico.

The temperature of the surface-water was taken every mile on all lines and in no case were there found any warm or cold bands. There was a slight rise of temperature on entering the current, at the surface, and also a corresponding rise at the bottom in the

same locality. The surface temperatures found in the Stream were much below those generally given in published works on the subject. The average temperature in the axis of the Stream rarely exceeded 83°F. in June and July. On one or two occasions the thermometer read as high as 86°, and once 89°, but it was at high noon in a dead calm. The temperature at five fathoms did not range above the average of 81½°. The surface temperatures did not indicate a cold wall inside of the Stream; the water between the one hundred fathom-line and the shore seemed to be an overflow of the Stream, as the temperatures at five, ten and fifteen fathoms were nearly as high as those found in the Stream.

Lines for series of temperatures from the surface to the bottom were run during the past summer from Block island to the Bermudas, and thence to Hatteras. The isothermals show the Labrador current until nearing the Stream, when they descend gradually and in the stream itself abruptly, to their greatest depths. Instead of the warm stream-water thinning away as it was reported to do when spread out, it was not much over fifty miles in width at the time of our crossing, as shown by the current and high surface temperatures. The temperatures below the surface were much higher than at the same depths off the coast.

The ordinary temperature at the bottom off Savannah and Charleston in 400 fathoms was 45°. In the Stream between Block island and the Bermudas it was as high as 55° at 400 fathoms. The isothermals remained at almost the same depth to the southward, as in the Stream, on the entire line to the Bermudas. Just north of the Stream the temperature at 400 fathoms was 39½° and 40°. At a point well in the Labrador current away from the Stream, the temperature at 400 fathoms was 38½°.

From the Bermudas to Hatteras the isothermals were at the same depths as were found south of the Stream on the previous line, but when in the current off Hatteras, where the Stream trends to the eastward, they rose to the same depth as off Charleston and Savannah on the plateau.

These temperatures below the surface seem to suggest that the Labrador current underruns the Stream at Hatteras, but at no other point. It probably keeps its natural boundary or western wall along the 1,000 fathom curve, and thus follows around the plateau towards the equator. No definite conclusion can be drawn

from the data obtained thus far; but we have at least a clear field to work upon in the future, in regard to temperatures and currents, and the contour of the bottom as far as Hatteras is well portrayed.

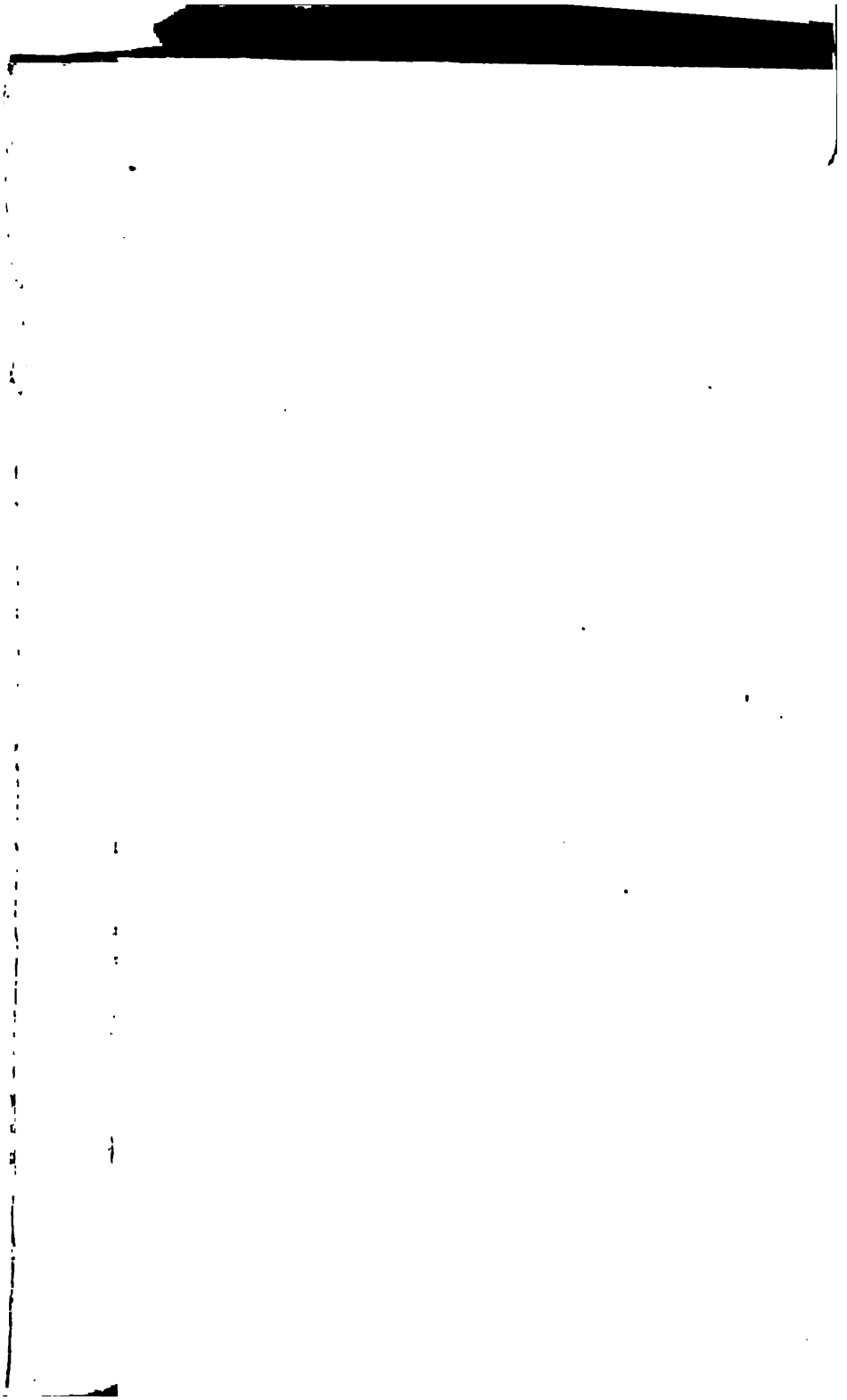
Tables are appended giving the depths, etc., found on the several lines.

Preglacial Channel of Eagle River, Michigan. By Chas. Whittlesey, of Cleveland, Ohio.

[ABSTRACT.]

It is to little purpose that an abstract of this paper is given without the illustrations. Before the glacial era, the Eagle River, at the Phenix Mine, on Keweenaw Point, ran on the back of a vein carrying native copper, which bears about north 20° west. The depth of the channel was forty to sixty feet, very straight, and as it wore away the vein matter, masses of copper were left at the bottom, the largest weighing nearly a ton. On Point Keweenaw the drift striæ are about north 50° east, nearly at right angles to the old channel, and as the ice covering moved southwesterly it pressed the gravel, sand and hard pan into the channel, with great force. It was not only filled, but covered, to the depth of thirty to ninety feet, with the material of the moraines.

By mere accident Mr. Martin Coryell, M. E., who was in 1845-6 superintending the mine, while driving an exploring adit in the rock from the bottom of a shaft, struck the ancient channel, the vein, and the copper boulders. The water of the present channel was running on the gravel thirty to forty feet overhead. About 200 feet northerly, the present river makes a right angle to the west at the foot of a high drift ridge which crosses its course. Mr. Coryell followed the ancient channel straightforward under the ridge which must have joined the present channel between the mine and Lake Superior. He left such plans of the underground work, that in 1854 I was able to construct a map of the two rivers which was presented to the section. Evidently there had been little loose surface material in the preglacial ages.



Recent Discoveries of Fossil Fishes in the Devonian Rocks of Canada. By J. F. Whiteaves, of Ottawa, Canada.[1]

[ABSTRACT.]

Deposits containing remains of fishes remarkably like those of the Old Red Sandstone of Scotland and Russia were discovered by Mr. R. W. Ells, M. A., of the Geological Survey of Canada, in 1879, at Scaumenac Bay, on the north shore of the Restigouche River and almost immediately opposite Dalhousie. Mr. A. H. Foord, of the same Survey, has since devoted the whole of the summer seasons of 1880 and 1881, as well as part of the present summer to a systematic exploration of these fish-bearing beds, from which he has succeeded in obtaining a large and instructive series of specimens. Prior to 1879 all the rocks at Scaumenac Bay were regarded as belonging to the Bonaventure division of the Lower Carboniferous, but the fossils recently collected show that at this locality the conglomerates and red sandstones of the Bonaventure series are underlaid by shales and sandstones of Devonian, and most probably of Upper Devonian age. The genera and species of fishes collected from these shales and sandstones may be thus briefly indicated.

1. *Pterichthys Canadensis.* Extremely abundant and first described under the above name in the American Journal of Science for August, 1880. It may possibly prove to be identical with the *Bothriolepis ornata* of Eichwald, from Scotland and Russia, but that species, of which only one or two isolated plates have been found, has never been defined with sufficient accuracy to be recognizable. The Canadian specimens show most conclusively that there is no essential difference between *Bothriolepis* and *Pterichthys*, a fact to which Dr. Pander had called attention in 1857. The divergent, slightly curved barbels on the front margin of the head, which are indicated in Agassiz's restoration of *Pterichthys*, but whose existence was doubted by Pander, are well seen in one of the Scaumenac Bay specimens. The singular flattened-conical dermal appendages of the helmet, one on each side of the orbit, and represented in Pander's more recent restoration of the genus, are equally well exhibited in two other specimens from the same locality.

[1] Palæontologist to the Geological Survey of Canada.

2. *Diplacanthus.* An apparently undescribed species, remarkable for the small size of each of its fin-spines in proportion to the general dimensions of the body.

3. *Acanthodes Mitchelli?* Egerton. A few examples of a diminutive Acanthodean, which seems to be barely distinguishable from the Scotch species named above. The entire length of the largest individual is not more than one inch and three-quarters.

4. *Phaneropleuron curtum.* A shorter and deeper form by far than the only previously known species of the genus, *P. Andersoni* of Huxley, from Dura Den. Dr. Gunther has shown that of all the Devonian fishes *Phaneropleuron* comes nearest to the living *Ceratodus Forsteri*, of Queensland, both in its internal and external characters, though the dentition of *Phaneropleuron* was very imperfectly ascertained. Last summer Mr. Foord was fortunate enough to obtain specimens of *P. curtum* at Scaumenac with the cutting and crushing teeth well preserved. The maxillary teeth are short, curved-conical, smooth and somewhat compressed. The palatal teeth, like those of *Dipterus*, as figured by Hugh Miller in the "Footprints of the Creator," are triangular in outline and bear parallel rows of short, erect, conical denticles, thus making the affinities of *Phaneropleuron* with the living *Ceratodus*, and to the Dipnoi generally, still more apparent.

5. *Eusthenopteron Foordi.* Many large fragments of a fish, which when perfect must have been more than three feet in length, and which appears to be the type of a new generic form allied to the *Tristichopterus* of Sir Philip Egerton, from which it differs only in the following respects. In *Tristichopterus* the vertebral centres are ossified, the osselets of the lower lobe of the tail spring from 8 or 9 interspinous bones, and the teeth (according to Dr. Traquair) are conical and circular in section. But in *Eusthenopteron* the vertebral centres are not ossified, the caudal osselets of the lower lobe of the tail are articulated to the dilated extremities of the hæmal spines, and the teeth are compressed conical with sharp lateral cutting edges. The entire jaw of *Eusthenopteron*, in fact, is singularly like that of the so-called *Asterolepis* of Hugh Miller, as represented in the "Footprints."

6. *Glyptolepis.* A single specimen, not very well preserved, of a small scaled species, which is very similar to and possibly identical with the *G. microlepidotus* of Agassiz.

7. *Glyptolepis.* A second species appears to be indicated in the collection by large, isolated scales fully one inch in length, belonging to a fish more of the type of the *G. leptopterus* of Agassiz.

8. *Cheirolepis Canadensis.* A fine large *Cheirolepis* nearly related to the *C. Cummingiæ* of Agassiz from the Old Red Sandstone of Scotland, of which it may prove to be only a variety. The sculpture of the scales on the body and fins, and the general outline, are alike in both, but in the Canadian species the position of the fins is different to that of those of its European representative.

On the south bank of the Restigouche, about half a mile above Campbellton, another series of fish-bearing deposits was discovered by Mr. Ells in 1881. These also have been carefully explored by Mr. Foord, and a description of the species contained in them has since been published in the "Canadian Naturalist." At this locality the fish remains occur in brecciated limestones of about the age of the Oriskany and Gaspé sandstones, and much disturbed by trappean outbursts and overflows. The specimens, though often well preserved, are generally fragmentary, and the species so far recognized are as follows:

1. *Coccosteus Acadicus.* Cranial shields and detached post-dorsomedian, ventromedian and preventrolateral plates of a species of *Coccosteus* in which the ornamentation of the head shield is like that of *C. decipiens*, and the shape of the post-dorsomedian plate is the same as that of *C. cuspidatus.*

2. *Cephalaspis Campbelltonensis.* Large head-shields of a *Cephalaspis* of the subgenus *Eucephalaspis*, which show the orbits and the prominences and depressions in their vicinity very clearly. In all the other species of the genus the sculpture of the exterior consists of minute raised tubercles, but in this one the enamel is very finely punctate.

3. *Ctenacanthus latispinosus.* Fin-spines only, characterized principally by their regularly nodose, radiating ribs.

4. *Homacanthus* (species undetermined). Fin-spines, also, of a species of *Homacanthus* which seem to differ from the *H. arcuatus* of Agassiz only in size, the Campbellton specimens being much larger than the European ones.

At Scaumenac the only other fossils found associated with fishes are land plants, which latter have been described by Principal Daw-

son. The plants as well as the fishes found in the Campbellton breccias are entirely different from those at Scaumenac, and at Campbellton, entomostraca, *Spirorbis Explanatus*, fragments of a large *Pterygotus* and two new species of *Cyclus* occur with the fishes.

The paper was illustrated by numerous specimens and by diagrams prepared by Mr. Albert Grignard, late draughtsman to the Canadian Survey.

Notes on the Occurrence of Siphonotreta Scotica Davidson in the Upper Silurian Near Ottawa, Ont. By J. F. Whiteaves, of Ottawa, Ont.

(Abstract.)

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"The *Siphonotreta* from near Ottawa interests me much. It is identical in shape and characters with the Upper Llandeilo species which I named *Siphonotreta Scotica*. I am very uncertain whether the Wenlock Shale species named *S. Anglica* by Morris is the same or not. Only one crushed specimen of the *S. Anglica* has been found and its spines are annulated as described by Morris. I could see no annulations in the spines of many specimens of *S. Scotica* found by Mrs. Gray in the Upper Llandeilo of Craighead, Ayrshire, nor do I see any in your specimens. As there is uncertainty as to the specific identity of the highest Upper Silurian form with the Lower Silurian one, and as none have been found in all that mass of intervening strata, I prefer provisionally to retain the two names, or until other Upper Silurian species shall have been found."

The interest of Mr. Watts' discovery lies of course in the fact that this is the first time that a species of the genus has been recorded as occurring in N. America.

On a Post-Tertiary Deposit containing Impressions of Leaves, in Cumberland county, New Jersey. By N. L. Britton, of New York, N. Y.[1]

[ABSTRACT.]

The deposit which this paper is intended to describe has long been known to a few geologists, but no detailed description of it has as yet been given. It is located near the city of Bridgeton, Cumberland county, N. J., near the shore of a large pond from which a stream flows and empties into the Cohansey Creek.

The material which bears the leaf-impressions is a rather coarse quartz sand, cemented together by hydrated sesquioxide of iron into a soft, friable sandstone. The stratum containing them is found immediately at the surface of the ground, apparently extending into the side of a low hill; it is at least four feet in

[1] Of Columbia College and assistant in the Geological Survey of New Jersey.

thickness, and in portions of it the impressions are very abundant, while at other points only an occasional one is to be found. The outcrop of this leaf bed may be traced for some twenty-five feet along the hillside. Its inclination is very nearly horizontal. This sandstone belongs geologically to the superficial formation which covers the Tertiary and Cretaceous strata of New Jersey, and also extends northward to some extent, as far as the terminal glacial moraine on the red shales and sandstones of the Trias. The materials composing this superficial formation are gravel, sand and loam, uniformly of a light color, generally yellow, white or gray, and from this circumstance it is known as the yellow drift; it is also termed the Pre-glacial drift, because it is older than the drift deposits due to glacial action.[2] This yellow drift is generally plainly stratified; it varies from forty feet or even more to a fraction of an inch in thickness, being sometimes represented only by a few yellow pebbles; it varies in coarseness from gravel with pebbles some inches in diameter to a fine sand or loam. It is often found that this material is cemented together into a reddish-brown sandstone or conglomerate by hydrated oxide of iron precipitated from springs since its deposition. This rock forms the only building stone native to the region and it is extensively used for lining wells, cellar walls, etc. A few buildings have been constructed of it, as the large seminary at Bridgeton. Casts of marine mollusks have been found in it at Tuckahoe, Atlantic county, where it contains casts of what were apparently the shells of the common hard clam (*Venus mercenaria*) and others, and I have recently secured specimens of other mollusks, mostly too poorly preserved for identification, in the yellow drift near Marlboro, Monmouth county. It also contains silicified fossil wood at Bridgeton, Shark River, New Egypt, Mullica Hill, and other localities.

The stratum bearing the leaf-impressions at Bridgeton is a rather coarse, soft sandstone — so coarse indeed that the margins of the leaves are often very indistinctly preserved making it difficult to determine the outline. The primary venation, however, and sometimes the finer subdivisions of the veins are easily to be seen.

The identification of the species represented is not yet completed. Among the genera we have *Liquidambar*, the sweet gum represented by several specimens of leaves resembling those of the

[2] For a full account of the Pre-glacial drift, see Annual Report of the State Geologist for 1880, pp. 87-97.

living species (*L. styraciflua* L.) but only three-lobed, instead of five- or seven-lobed as the leaves of that tree are; numerous specimens of a species of *Viburnum*, as shown by the venation; fragments of a broad-leaved grass, perhaps *Zizania;* serrated leaves of a species of *Ulmus*, and several other genera.

This is the only locality known in New Jersey where the preglacial drift contains leaf-impressions. The species will be figured and described in reports on the fossil floras of New Jersey, now in preparation.

TERRACES AND BEACHES ABOUT LAKE ONTARIO. By J. W. SPENCER, of Columbia, Missouri.

[ABSTRACT.[1]]

IN and about the Dundas valley, at the extreme western end of Lake Ontario,— topographically described by the author in his paper on the Origin of the Lakes, in the Proceedings of this Society for 1881,— there are the remains of a considerable number of terraces and beaches.

The lowest of these beaches is that forming the modern western shore of the lake. Burlington beach, which separates Burlington bay (a body of water about fifteen miles area and seventy-eight feet deep) from Lake Ontario, rises only eight feet above the water. It is composed wholly of sand and gravel (containing an abundance of fossils belonging to the Hudson river formation) which have been transported by ice and wave action, a distance of at least twenty or thirty miles, from a region northwest of the lake, along with a few crystalline pebbles. This beach and shore line contains no Niagara pebbles, although almost beneath the Niagara escarpment.

In elevation, the next terrace is south of the Dundas marsh, at

[1] For the minute details of the subject of this paper, the reader is referred to the author's lengthy paper on the "Surface Geology of the Region about the Western End of Lake Ontario," in the Canadian Naturalist for 1882, copies of which can be obtained from Dr. Spencer. See also American Journal of Science for Dec., 1882.

from 70 to 80 feet above the lake, and forms a plane of considerable area, made up of fine, thinly bedded, arenaceous clay.

The third and most conspicuous terrace is that at 116 feet above the lake and extends as a contour line, for long distances about the western end of the lake. It forms "Burlington height" which separates what is now the large Dundas marsh (at the level of the lake) from the lake. It is composed of the same material as the present lake shore—Hudson river sands and pebbles, without any admixture of Niagara pebbles.

Again in the Dundas valley we have isolated remains of former stratified shore lines at 180, 224, 261, 335, 440 (?), and 500 feet above the lake. All of these higher beaches are composed of sand and pebbles of the Hudson river period, with less or more *débris* of the adjacent Niagara formation. In fact, the study of the beaches about the western end of Lake Ontario shows a gradual recession of the water of the lake, with few sudden changes of level. Also the transportation of the greater proportion of the material of all the beaches was from the country northwest of the lake, and from lower elevations than those of the old shore line.

Between Toronto and Lake Simcoe, Mr. Thos. Roy measured beaches at 110, 210, 282, 310, 346, 402, 422, 502, 558, 626, 682, 734, and 764 feet respectively above Lake Ontario. In addition to these gravel beaches, others, at 600 feet, and on descending towards Georgian bay (along the Northern Railway) at 520, 388, and 354 feet, have been measured.

Along the Toronto, Grey and Bruce Railway, which extends in a direction north of westward from Toronto to and across the highest portions of the peninsula of Ontario, underlaid in part by the "Artemesia gravel" ridges, there are a number of conspicuous sand and gravel beds, which follow contour lines more or less closely. Edmund Wragg, Esq., furnished me with the following railway levels of many of these beds. They have been measured at 160, 280, 370, 710, 990, 1,120, and 1,340 feet above Lake Ontario. After crossing the summit of the road at 1,462 feet, there are extensive gravel beds at 1310, and from 1000 to 697 feet above the same datum along the main line of the railway, and along the western branch of the road, at 1299, 1130, 1050, 870, 850, and 830 feet above Lake Ontario. Near Owen Sound there are others at 546, 496, and 466 feet above Lake Ontario.

Along the Great Western Railway, in the valley of St. Davids, the interglacial outlet of the Niagara river is filled with stratified sand and gravel of the Hudson river formation from about 250 feet to 380 feet above the lake.

The elevations of terraces in New York State are imperfectly known to me. Eastward of Lockport there are the lake ridges from 158 to 190 feet above the lake (Hall). In the region of Seneca lake and Skaneateles lake, I have observed beaches of various heights to about 625 feet above Lake Ontario. In Michigan, there are beaches to 1350 feet above Lake Ontario (Rominger). Near Petit Écrit, Lake Superior, there are beaches at 398, 408, 458, 592, 627, 635, and 699 above the same datum (Geol. of Canada). Again, southwest of Lake Erie, there are beaches or ridges at 65, 90, 165, 195, 220, 350, 408, 386–490 feet above Lake Erie (Messrs. Gilbert and Winchell).

In the Ontario peninsula, the outline of the Artemesia gravel may be approximately represented by the 1250 foot contour line, though extending south of it. This great belt of gravel is generally parallel with the Niagara escarpment, and is mostly composed of Guelph dolomites, or, in some localities of *débris* from adjacent rocks. There are more or less fragments of Laurentian and Huronian crystalline rocks, intermingled with the other well waterworn material. Indeed, there are often rounded fragments of Hudson river pebbles (these coming from lower elevations).

From the eastern side of the Artemesia gravel a narrow ridge extends for nearly a hundred miles, nearly parallel to, and only a few miles distant from, Lake Ontario, known as "Oak Ridge." Its most conspicuous portion may be represented by the contour line of 650 feet above Lake Ontario, although the highest portion rises 893 feet. This ridge has an elevation of from 200 to 300 feet above the broad rock-bottomed trough on its northern side, which extends from Georgian bay to the eastern end of Lake Ontario. A spur of this ridge extends southward to Lake Ontario at Scarboro heights, a few miles east of Toronto.

There is a resemblance between the Artemesia ridges and the so-called Kettle moraines of Wisconsin, Coteau des Prairies and Coteau de Missouri. Between these ridges there is a general parallelism. The Artemesia gravel reaches 1700 feet above the sea—a height as great as portions of Coteau des Prairies.

The deposit of the Artemesia gravel is simply around the high

rocky floor of a portion of country, and marks the recession of the waters, in more or less perfect contour lines, with most of the material of local origin and stratified.

Whatever barriers may have separated the lake region from the sea, there seems no doubt that the whole area was submerged beneath the sea level to at least 1700 feet, for no glacial lake could account for the high level beaches.[2]

As the continent was rising the waters of this inland lake had many channels communicating with the exterior sea, across Ohio and New York, besides that by way of the St. Lawrence valley. Local oscillation doubtless played an important part in letting out the confined waters, but to what extent, it cannot yet be determined. Below 1200 feet above the sea-level of to-day, the principal old outlets are by the valley of Cayuga lake, at 1015 feet; by the valley of Seneca lake, at 865 feet; by the Mohawk river, at 434 feet, and by the present St. Lawrence valley, at 247 feet. Dr. Newberry enumerates various other outlets in Ohio, at 936, 968, 909, 910, and 940 feet above the present ocean level.

There is a marked connection between some of these old outlets, and the beaches which rise a few feet above them, in that they are the most conspicuous and widespread.

The explanation of much of the transported material, and especially the rocks of Hudson river formation, at localities situated higher than their original sources, can best be accounted for by the theory that they were carried upward during the time when the continent was undergoing subsidence, and that they were re-arranged by the waves and shore ice of a subsequent period.

Let us now consider the origin of *Burlington beach* and *Burlington heights*. These beaches, separating bays from the lake at different times, owe their origin to common causes. The materials for both of these beaches (whether at 8 feet or 116 feet above the present lake), were brought some distance from the northwestern side of the lake, and not from local material; for had such been brought down, their composition would be pebbles of Niagara rocks in place of those of the Hudson river formation. Moreover, the transporting power, and also the quantity of water of the

[2] Since the paper of which the above is an abstract was written, Dr. Dawson, the President of this Association, has described the occurrence of the remains of a large whale, in gravels northeast of Lake Ontario, at an elevation of 440 feet above the sea—thus proving that during the formation of some of the widespread beaches of the lake basin, there was direct connection with the sea.

streams of the Dundas valley, had become very unimportant before any of the deposits succeeding the interglacial epoch. Formerly, for the higher ridge, when the lake was 116 feet above its present surface, as afterwards for the present Burlington beach, the material was transported by shore ice and wave action to the present position of the deposits, at a time when the winters need not have been colder than at present. The pebble laden ice would naturally get piled up by the action of the winter waves (as at the present day) at the more confined end of the lake, and eventually the ice would deposit its load to form beaches. The bays at the end of the lake are now frozen over in winter, while the lake has often no more than ice-fringes. As the streams now entering the lake all do so through marshes, there would not be the slightest power from them to act against the waves to produce the present beach separating Burlington bay from the lake. Moreover, the action which built up Burlington heights moved from the open lake, as shown by the inclined bedding of the deposits.

Occurrence of Graptolites in the Niagara Formation of Canada. By J. W. Spencer, King's College, Windsor, N. S.[1]

[ABSTRACT.]

The plant-like forms, that are included in the Graptolite family, have hitherto been regarded as essentially belonging to the Cambrian and Cambro-Silurian ages. They are generally preserved in the more muddy deposits of these eras. However, there has been discovered a Canadian locality in the Niagara formation, containing a graptolitic fauna, almost as large and variable as that of the Quebec group.

In the Palæontology of New York, Professor Hall described two species of Graptolites from the Clinton shales and three species from those of the Niagara formation. Subsequently, Professors Hall and Whitfield described one other species found in

[1] Since the above paper was read, the author's address is changed to State University, Columbia, Mo.

Ohio, and Principal Dawson another occurring in Nova Scotia. The rich graptolitic fauna of the Niagara formation was discovered by the careful and continued search of Col. Charles Coote Grant (H. P. Sixteenth Regiment) at Hamilton, Ontario. From the material obtained at that city, principally by Col. Grant, I have been enabled to study out twenty-nine new species, in addition to those previously known, the whole fauna representing twelve genera, which lived in the Niagara and Clinton seas. Several other species doubtless exist, but their remains are in too fragmentary a condition for determination.

The graptolites at Hamilton occur mostly in nearly non-bituminous limestones, whose structure, under the microscope is seen to consist of recrystallized grains of dolomite. However, some of the best preserved specimens are obtained from the more shaly limestones. Owing to the condition of preservation, although the external forms are often perfect, and the chitinous substance in excellent condition, yet the cellular structure is exhibited in only occasional specimens. It sometimes happens that the impressions of the cellular orifices are well shown on the surface of the stone where the carbonaceous matter has been removed. In the study of these forms it has frequently been necessary to depend upon the relative sizes and modes of branching in order to determine the species. In many cases the solid axes of the stipes are shown to be lateral while in others they are central — a most important character.

The geological distribution is given by Professor Hall as follows: one species from the Potsdam group; fifty-three species, of twelve genera, from the Quebec group; four, from the Trenton; thirty, of eight genera, from the Utica and Hudson groups; two, from the Clinton; three, from the Niagara; one, from the Cornif-erous; and one, from the Hamilton group.

In the Canadian Naturalist for 1878, I described nine new species and three new genera. Of these genera, *Calyptograptus* closely resembles *Dictyonema*, with few or no cross-bars, but with branches overlapping. I have seen one species from the Niagara group of Kentucky (locally included in *Dictyonema*), and one from the Hudson group near Cincinnati, which evidently belonged to this genus. Another new genus then described was *Acanthograptus*, nearly related to *Inocaulis*, and the third was *Rhizograptus*, a cyathiform frond with slender axis and large bulbous

root. A fourth genus I have recently called Cyclograptus, which consists of a small circular frond, composed of a central disk and numerous short celluliferous stipes. (Types of the new genera were here exhibited).

The specific arrangement of the Niagara graptolites in genera is as follows:

Phyllograptus,	. .	one species.
Retiolites,	. . .	" "
Dendrograptus,	. .	six "
Callograptus,	. .	four "
Rhizograptus,	. .	two "
Acanthograptus,	. .	two "
Inocaulis,	. . .	seven "
Thamnograptus,	. .	two "
Ptilograptus,	. .	one "
Cyclograptus,	. .	" "

besides one species of *Dictyonema*, and one of *Inocaulis* not found at Hamilton.

The study of these organisms is completed as far as possible up to the present time, and in the course of a few months I hope to have it published in Part 3 (Palæontology) of the "Geology of the Region about the Western End of Lake Ontario."

A Mastodon Americanus in a Beaver Meadow. By Samuel Lockwood, of Freehold, N. J.

[ABSTRACT.[1]]

Professor Lockwood, in his paper, gave the particulars of his discovery and exhumation of the remains of a mastodon in a peat meadow about two miles west of Freehold, N. J. He had previously discovered in a peat-bog near the same village a fossil beaver-

[1] Published in full in the Montreal Herald, Aug. 25, 1883. Also in the Popular Science Monthly, January, 1883.

dam, which enabled him to detect some resemblances, and thus to suspect that this mastodon had been actually buried by beavers. This suspicion became an induction warranted by the facts which the digging disclosed, for fragments of beaver sticks were found lying across the neck of the skeleton, which lay on its right side, the tusks being deflected under the neck. The remains lay on the "hard pan" beneath the peat. It was shown that the monster died on the right bank of the ancient stream, that the beavers afterward dammed it and the pond covered the skeleton. In due time the pond was abandoned and the dam wall, as it sunk down by decay, drained the pond. As its area decreased, the area of swamp increased, and a growth of sphagnum peat buried the bones. The drainage finally became such that a meadow resulted.

The writer mentioned three instances of his finding in Monmouth County mastodon relics on the coast. Twice at long intervals were teeth obtained out at sea from Long Branch. In the other instance a part of a tusk and other bones were got from an ancient buried swamp about fifteen miles south of Long Branch, which swamp was uncovered by a severe storm from the sea.

The points made in the paper were: (1) The presentation for the first time of the evidence of the mastodon for the subsiding of a part of the eastern coast-line of the United States. (2) The long life range of the mastodon. Beginning with the Pliocene, it was shown that the individual in question was living at a period well up into very recent geologic time. (3) The mastodon came in with the great extinct fossil beaver, which it outlived, and became contemporary with the modern beaver. (4) The mastodon lived to be contemporary with the American autochthonic man, and, probably as a food supply, before him this Behemoth melted away.

Dr. Lockwood said that the tusks which were uncovered in perfection must have contained 400 pounds of ivory, but these and the bones, excepting four teeth, literally crumbled away on reaching the air, to his inexpressible grief.

CURRENTS OF AIR AND OCEAN IN CONNECTION WITH CLIMATES, REGIONS OF SUMMER RAINS AND SUMMER DROUGHTS. By J. BEAUFORT HURLBERT, of Ottawa, Canada.

[ABSTRACT.]

THE prevailing winds in the north temperate zone are from southwest towards the northeast. This great current of air may be said to be constant north of latitude 35°. In the upper region of the air it blows nearly every day in the year from some point near the southwest towards the northeast. With a constant movement of the air in high altitudes from the southwest there must be a return current from the north towards the southwest, as there are counter currents in the ocean; but these polar winds near the surface of the earth blow from all the colder points of the compass.

The warm currents of air and water falling upon western coasts, and aërial currents passing over the continents, elevate the temperatures of the western parts of the continents, while the cold currents pressing upon eastern shores lower the temperatures there.

The mean temperature of the Gulf stream in the Gulf of Mexico is 80° Fahrenheit; its maximum temperature is 86°, or 9° above the ocean temperature due the latitude. Increasing its latitude 10°, it loses two degrees of heat, and after running 3,000 miles towards the north still preserves the temperature of summer. With this temperature it crosses the fortieth degree of north latitude, and spreading out for thousands of square leagues over the cold waters of the ocean, does much to mitigate the rigors of winter in Europe. When it strikes the British islands it divides into two parts, the main current going to the Polar sea, the other entering the Bay of Biscay.

It has been estimated that the quantity of heat discharged over the Atlantic, from the waters of the Gulf stream in winter, would be sufficient to raise the whole column of atmosphere which rests upon France and the British islands from the freezing point to summer heat. Every western wind which blows (and the prevailing winds are from the west or from some point near the west or southwest in this part of the ocean) crosses the Gulf stream and carries with it a portion of its heat, discharging it in its pas-

sage over Europe. The isothermal lines of 60° and 55°, starting from the parallel of 40° on the American coast, run in a north-easterly direction, retaining nearly the same oceanic temperature on the European side in latitude 55° and 60° as exists on the American coast in latitude 40°.

In the Pacific there are tropic and arctic currents like those in the Atlantic, and from similar causes. The Japan stream, or Kuro-Siwo — black stream — a name derived from the deep blue color of its waters, flows from the southeast of Asia in a north-easterly direction, falling upon the western coast of North America. This stream, flowing many thousand miles further than the Atlantic tropic current, is not so hot nor its littoral waters so cold as those in the Atlantic, but it spreads over the entire Pacific coast of Canada. These two currents in the Pacific — the arctic and tropical — produce similar effects to those in the Atlantic; the one warming the western coast of North America, in high latitudes, and the other cooling the eastern shores of Asia.

Through the agency of these two currents in the Atlantic, the western countries of Europe are much warmer than the eastern parts of America in similar latitudes; the difference being about eight degrees in latitude 41°; eleven and a half in latitude 51°; and twenty-five in latitude 58°. Similar causes in operation in the Pacific ocean give an equal elevation of the temperature of the western coasts of America over the eastern coasts of Asia in the same latitudes — the arctic currents chilling the one and the tropical currents warming the other.

From Vancouver in latitude 49° to Sitka in 57°, the summer temperatures are as high and as uniform as in the west of Europe, except where the vicinity of mountains may modify the normal conditions of climate. Sir John Richardson says "the climate of Sitka" (on the Pacific coast) "is much warmer than that of Europe in the same parallel" (Arc. Ex., Vol. II, p. 279).

The isothermal of 60° for the three summer months rises as high as latitude 63° east of the Rocky mountains in the valley of Mackenzie river. Youkon, west of Mackenzie river and within the Arctic circle, latitude 67°, has a July 65° 7′, and an August of 60°.

In comparing the well known regions of the old world with the less known corresponding parts of the new, western coasts with western, eastern with eastern, and interior divisions with interior,

on the north. The winds over these desert areas on both continents blow almost invariably in summer from some point near the southwest towards the northeast. These portions of the continents are destitute mainly of summer rains, but have high summer temperatures. Upon the modifications caused in these desert areas by the high lands of Mexico and the vast Mediterranean sea we cannot here enter, but may merely state that the mountains of Mexico limit the deserts there, and that great sea north of Africa causes a more humid air in the south of Europe and limits the deserts in that direction; yet Spain, Italy, Sicily, and the whole country northeastward into Hungary, frequently suffer from summer droughts.

The reason, as a permanent cause, often assigned for these portions of the continent being destitute of trees—namely, the prairie fires — cannot for a moment bear investigation.

Fires were naturally suggested to the first rude settlers, to whom the evidence of sight is the chief guide, as the only cause. In many parts of the African, Asiatic and American deserts and prairies there are no fires, still they are treeless. In other countries, fires are as frequent where forests are permanent, or, if burned down, young trees immediately grow up again. The existence of forests over a region 2,000 miles by 1,000, and their failure *where, and only where*, the summer rains fail and the arid winds prevail, ought to have suggested the explanation.

That the prairie fires sweeping over extended areas may have kept trees from some localities, near rivers or on retentive soils, is possible; but such exceptions, limited and local, have no weight in opposition to the fact that millions of square miles have remained, through all the ages of history, desert and treeless on the borders of other equally extensive areas covered with dense forests. The climates which have produced these two distinct results over those regions have remained permanent for ages, and will remain permanent in the future, unless changes supervene in the entire solar system; but for the calculation of such imaginary phenomena astronomy furnishes no data. We may infer, therefore, that those conditions of climate — heat and humidity in the one case, and heat and aridity in the other — remaining the same, their effects — forests and treeless regions — will be permanent expressions of those fixed efficient causes.

The attempts to account for the deserts of the Old and New

World by the physical configuration of the continents, have led to many ingenious theories. The chief of these is the assumption that mountain chains to the west of those rainless regions condense the vapor brought in the southwest winds, causing heavy rains on the western sides of the mountains, but leaving the winds without vapor east of these mountain chains. The phenomena of rain on the west, but none on the east of the Ghants in Hindostan and other places, have been taken as sufficient bases for this theory.

That those regions having no summer rains are on similar parts of the continents north and south of the equator, both in the eastern and western hemispheres, beginning on western coasts at about the same latitudes; that there are no mountains west of the Desert of Sahara, and the rainless regions in Australia; that the entire coasts of Mexico, old and new California, west of the mountains, are quite as destitute of rain in summer as the regions east; that those west winds give heavy falls of rain to the north and northeast throughout Canada, and in autumn, winter, and early spring, deposit heavy rain and snow on those interior desert areas east of the Rocky mountains, over which they are said to pass in summer, devoid of vapor,—these and other facts ought to have corrected the erroneous opinions on this subject.

That the great southwest currents of air — the tropical currents —bring the vapor which falls in rain and snow, is here assumed as admitted, for it is the basis of the theory which we are controverting.

Those vast wastes are, at the season of the year when little or no rain falls, highly heated by the summer suns. The southwest winds passing over these become rarefied, and thus, being capable of sustaining even more vapor than at a lower temperature, retain the humidity with which they have come from the tropics so heavily charged, till they reach the cooler regions, north and northeast, where they are condensed into rain or snow. Hence in summer the line dividing the zones of rain from those of drought is farther to the northeast; as autumn and winter approach, those highly heated plains gradually cool sufficiently to condense the vapors in the southwest winds, which now give heavy depositions of rain and snow.

A Source of the Bituminous Matter of the Black Shales of Ohio. By Edward Orton, of Columbus, Ohio.

The great black shale of Ohio, that extends from the Huron river on the shore of Lake Erie, to the mouth of the Scioto river in the Ohio valley, with an outcrop varying from ten to twenty miles in breadth, I have already shown to be a composite stratum. In this determination, I have followed Newberry, who showed in his reports upon the geology of northern Ohio that the two black shales of Cuyahoga county, for example, the lower or *Huron* of his nomenclature, and the upper or *Cleveland*, which are there separated by a wedge of greenish blue, *Erie* shale, when followed to the westward, coalesce, through the failure of the intervening member. In his view, the Cleveland shale comes down by overlap upon the Huron, so that the black shale of Huron county consists of the blended products of the two or three formations already named. This stratum I have followed not only with no hint of interruption or division, but with manifold proofs of unbroken continuity, across the state. The name and geological place of the stratum, as assigned in the volumes of the last geological survey of the state, will both require modification according to this view. It can no longer be called the *Huron* shale, for this element constitutes only its lower portion. The boundary between Carboniferous and Devonian time can no longer be drawn at the line of junction of the Erie and Cleveland shales, for that line has disappeared, and the Cleveland shale is only the upper portion of 350 feet of black shales through which thus far no planes of division have been established.

As to the first point, I know no better name for the present, than the old one, viz.: the *Ohio Black Shale*. Professor Shaler, in his reports, upon the geology of Kentucky has reverted to this designation.

As to the place of the stratum in the geological column, I believe that it will be found necessary to refer it all to one age, viz., to the Devonian. The greater part is unquestionably of this age and for the balance, the fossils are too few, through most of the state, and too indecisive in character, to make any proper ground for establishing so important a division. I will, therefore, designate this stratum the Ohio Black Shale and I will, provisionally

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Pike county for more than two years. Some of the beds burn with a blaze.

It is this same organic matter, obviously, to which they owe their character as oil-producing shales. Oil and gas springs are constantly associated with their outcrops. They have already been turned to account for the artificial production of oil in Ohio and it is scarcely to be doubted that the great stock of carbon which they contain will at some time be utilized by man.

This organic matter has been referred both to animal and vegetable sources for its origin, and both these divisions of the living world have certainly contributed to it, for the remains of both are found in the shales, but the two factors are by no means of equal value.

The clearest and most extended statement that I know in regard to the accumulation of the bituminous matter of these shales is that made by Newberry in volume I, Geology of Ohio, page 156. He speaks of the Huron shale particularly, but what he says will apply to all of the divisions already named. I quote his words: "The nature of the sediments which form the Huron shale, the exceeding fineness of the mineral matter, the large percentage and uniform dissemination of carbon, the peculiar composition of this carbonaceous element, consisting mainly of hydrocarbons, the abundance of marine plants and the absence of terrestrial vegetation, all combine to show that it was deposited in an open sea and not immediately adjacent to the land."

"Under such circumstances we are compelled to attribute the carbonaceous matter to marine vegetation or to marine animals. If it were derived from marine animals, such as we know have contributed largely to the organic constituents of some rocks, the bituminous limestones for example, we should find much more abundant traces of their structures than we now do, since they are, in fact, almost entirely absent. We are, therefore, compelled to consider this accumulation of bituminous matter as the result of the growth of seaweeds in marine basins."

"Most marine plants with which we are acquainted grow upon the shore or in shallow water, and we can hardly imagine so large an area as that occupied by the Huron shale covered with a growth of shallow-water plants without its bearing evidence in some locality of shore lines. It is true, however, that a great growth of aquatic vegetation sometimes takes place remote from the land and where

at least, refer it to the Devonian age. Its approximate area I have already indicated, a belt of ten to twenty miles in breadth, extending across the state from north to south or for nearly two hundred miles. Its thickness ranges along its line of outcrop, from 250 to 350 feet.

There is another stratum of black shale in the Ohio column, of small thickness but of great persistency and extent. It lies about one hundred feet above the summit of the Ohio shale. It was first distinctly recognized and described by the late Professor E. B. Andrews, who called it the *Waverly Black Shale*. In southern Ohio, its boundaries are very distinct and definite. It is borne by the lower Waverly quarry stone, which is the continuation of the Berea Grit, for that portion of the state. The shale comes directly down upon the quarry stone. Its upper boundary is equally abrupt. It has a thickness in this region ranging from fifteen to thirty feet. Throughout central Ohio, the same description will apply, and no change is needed for the northern portion of the state except that the shale here is not always as dark in color as elsewhere, and its upper boundary is not distinct. It makes the roof of the Berea quarries, as it does of the same stratum everywhere to the southward. Meek separated it by implication from the overlying Cuyahoga shale in describing the fossils that it contains, giving to it the name of *Berea Shale*. Newberry describes it clearly but does not separate it from the Cuyahoga shale. It is charged with fossils, several of which seem characteristic.

These three beds of bituminous shale, viz., the Huron and Cleveland of Newberry and the last-named, the Berea shale, have many points of structure and history in common. They are all marine in origin, as the presence of their fossils attests. They were all formed in quiet waters and not upon shore lines, as their fine and undisturbed lamination shows. But their chief point of agreement is in the fact that is implied in one of their most common designations. They are all *bituminous* shales. According to the analyses with which I am acquainted, they carry from eight to twenty-two per cent of organic matter. It is this that gives them their color. It also renders them combustible, to a greater or less degree. There is scarcely a summer in which some of the shale banks of southern Ohio do not take fire from burning brush heaps and the like. When the conditions favor, these fires may continue for weeks or months. One is said to have lasted in the hills of

has been unsuccessful. It is reasonable to suppose that the organisms contained no silica nor lime, and that in their decomposition and bituminization, all organic structure was destroyed."

Within the last year, I have discovered in the black shales of Ohio, several microscopic forms of vegetation which are found in the shales in vast numbers and which obviously constitute an important source, and, as I believe, the chief source, of the bituminous matter that characterizes the formations. A brief account of the facts, I desire to present at this time to the Association.

In the spring of 1881, I had some correspondence with Mr. J. A. Flickinger, ex-county surveyor of Ashtabula county, Ohio, in regard to a deep well, the drilling of which was in progress under his supervision, near the village of Kingsville, in the county above named. A result of the correspondence was that Mr. Flickinger kindly furnished to me the drillings that he had saved from different depths as the work had gone forward. Samples were saved every day or two and always when any change was reported in the materials through which the drill was passing. Fifty or more specimens were furnished, representing a range of between 1200 and 1300 feet.

The record that they gave was plain to read. The boring began in the Erie shale of Newberry and continued in it, with but minor changes of composition for nearly 1000 feet, where the greenish blue layers of the Erie were replaced by the black beds of the Huron. On examining with the microscope the fragments of the latter that were first reached, I found several of the samples, and notably those at 996, 1016 and 1094 feet to contain minute translucent disks, resinous in appearance and unmistakably organic in origin, in great numbers. Beyond this point, I made no progress for a number of months, until upon a microscopic examination of the black shale from Columbus, accidentally undertaken, I found it also charged with forms similar to or perhaps identical with those already recognized from the buried shale of northern Ohio. I then made examination of typical representatives of the several black shales already named throughout the state, the Huron shale from the mouth of Huron river, the Cleveland shale from Cleveland and other points in the vicinity, the Berea shale from the Ohio valley. I also examined the outcrops of the great black shale throughout central and southern Ohio, taking samples from every portion of the series, bottom, middle and top. The result of these

the plants have no attachment to the bottom. Of these areas, we have a type in the Sargasso Sea, through which Columbus ploughed his way, when making the voyage that resulted in the discovery of America. There, as in other similar sheets of seaweed, the vegetation floats upon the surface of the water and maintains a vigorous and luxuriant growth without connection with shore or bottom. Corresponding to this growth must be the decomposition of vegetable tissue on a large scale. The products of such decomposition would fall to the ocean bottom as finely comminuted carbonaceous mud, mingled with stems and fronds detached by violence or decay. Under all such sheets of vegetation, in a sea where a fine mechanical sediment is being deposited, we must necessarily have an accumulation of mud, containing a large percentage of carbonaceous matter; in other words, the elements of a bituminous shale."

"Waiting the demonstrative solution of the problem which patient and exhaustive study will doubtless sometime furnish, I offer as a possible explanation of the peculiar features of the Huron shale, the suggestion that its carbon was derived from vegetation which lined the shores and covered the surface of a quiet and almost land-surrounded sea."

This statement is expanded almost to the proportions of a theory as to the origin of the black shales. In substance, it refers the bituminous matter of the shales to marine vegetation, the decomposition having been carried to such a point as to destroy almost all traces of structure.

A similar view was briefly expressed by the late Professor Andrews, in the Report of Progress of the Ohio Geological Survey for 1869. On page 65, he says, "the conditions under which this formation" (viz., the Ohio Black Shale) "was deposited, involved comparatively quiet waters, charged with a constant supply of fine sediment with which there was at all times commingled organic matter which alone could have furnished the bitumen. The even distribution of the bitumen throughout the entire mass of the sediments would imply that the water abounded with the minute forms of vegetable or animal life. Thus far, search for their forms has been unrewarded. After a failure by myself, I placed samples of the slate in the hands of Professor Wormley, whose skill in microscopic researches is well known and whose instruments are of the most perfect kind. Thus far his search for distinct organisms

has been unsuccessful. It is reasonable to suppose that the organisms contained no silica nor lime, and that in their decomposition and bituminization, all organic structure was destroyed."

Within the last year, I have discovered in the black shales of Ohio, several microscopic forms of vegetation which are found in the shales in vast numbers and which obviously constitute an important source, and, as I believe, the chief source, of the bituminous matter that characterizes the formations. A brief account of the facts, I desire to present at this time to the Association.

In the spring of 1881, I had some correspondence with Mr. J. A. Flickinger, ex-county surveyor of Ashtabula county, Ohio, in regard to a deep well, the drilling of which was in progress under his supervision, near the village of Kingsville, in the county above named. A result of the correspondence was that Mr. Flickinger kindly furnished to me the drillings that he had saved from different depths as the work had gone forward. Samples were saved every day or two and always when any change was reported in the materials through which the drill was passing. Fifty or more specimens were furnished, representing a range of between 1200 and 1300 feet.

The record that they gave was plain to read. The boring began in the Erie shale of Newberry and continued in it, with but minor changes of composition for nearly 1000 feet, where the greenish blue layers of the Erie were replaced by the black beds of the Huron. On examining with the microscope the fragments of the latter that were first reached, I found several of the samples, and notably those at 996, 1016 and 1094 feet to contain minute translucent disks, resinous in appearance and unmistakably organic in origin, in great numbers. Beyond this point, I made no progress for a number of months, until upon a microscopic examination of the black shale from Columbus, accidentally undertaken, I found it also charged with forms similar to or perhaps identical with those already recognized from the buried shale of northern Ohio. I then made examination of typical representatives of the several black shales already named throughout the state, the Huron shale from the mouth of Huron river, the Cleveland shale from Cleveland and other points in the vicinity, the Berea shale from the Ohio valley. I also examined the outcrops of the great black shale throughout central and southern Ohio, taking samples from every portion of the series, bottom, middle and top. The result of these

[illegible]

the bodies themselves or perhaps even more strikingly by the pits from which they have been withdrawn. Many of the latter show impressions and reliefs that resemble the track of a newly-shod horse.

What seems to be a suture or opening, occupying about one-fourth of the circumference is a common feature, and a pit for the attachment of a stem can be recognized in very many. In a few, rare instances, *the stem itself is found with the disks attached?*

These constitute the general and non-professional statements that it seems necessary to make. I submit a few drawings of the forms in question, made by one of my assistants, for the accuracy of which I can vouch. Better still than drawings, however faithful, are the objects themselves. I have a number of pieces of the shale which I shall be glad to distribute to any who care to examine them.

What are these forms and from what sources are they derived? A botanist must answer these questions when they are fully answered, as I believe. My own interpretation was substantially determined by the appearance that microscopic sections of the shale present, viz., yellow translucent circles, in sections parallel to the bedding, and elongated bars or much flattened hoops when cut transversely. These facts correspond so closely with the description of spore-cases in coals as given by Williamson and Huxley that I at first believed the forms to be lycopodiaceous sporangia. They did not, however, show the trigonal form that belongs to the spore-cases of most lycopods and certain facts that I afterwards discovered seemed to militate against this view. That they are either sporangia or spores, there seems no reason to doubt, but the facts so far accumulated would seem to indicate groups somewhat below lycopods in rank as their sources. Upon this point, I am not entitled to an independent judgment. Hoping that this interesting material may be taken up by a competent investigator, I await results.

I have spoken all along as if I were the original discoverer of these fossils of the shale. In the sense that I reached the facts without suggestion or aid from other sources, the discovery is my own, but when I came to correspond with our authorities on paleobotany, I learned from Dr. Dawson, and with very great satisfaction, that he had already had his eye upon these forms, that he had not only recognized but had figured and described, ten years ago, one of the most abundant of them. In an article entitled "*Spore Cases in Coals,*" in the April number of the American

Journal of Science for 1871, the distinguished author figures and briefly describes a form that seems, from specimens which he has kindly furnished to me, identical with the one which I at first recognized, that came from 1000 feet below the surface in northern Ohio. This account is brief and I will give it entire.

"The oldest bed of spore-cases known to me is that at Kettle Point, Lake Huron. It is a bed of brown, bituminous shale, burning with much flame and under a lens is seen to be studded with flattened disk-like bodies, scarcely more than a hundredth of an inch in diameter, which under the microscope are found to be spore-cases, slightly papillate externally, and with a point of attachment on one side and a slit more or less elongated and gaping on the other. I have proposed for these bodies the name *Sporangites Huronensis*. When thin slices of the rock are made, its substance is seen to be filled with these bodies which, viewed as transparent objects, appear yellow like amber and show little structure except that the walls can in some instances be distinguished from the internal cavity and the latter may be seen to inclose patches of flocculent or granular matter. In the shale containing them, there are also vast numbers of rounded, translucent granules which may be the escaped spores."

"The bed at Kettle Point is stated in the report of the Geological Survey to be twelve to fourteen feet in thickness, but to what degree either in its thickness or horizontal extent, it retains the characters above described, I do not know. It belongs to the Upper Devonian, being supposed to be a representative of the Genesee shales of New York. It contains stems of *Calamites inornatus*, and of a *Lepidodendron*, obscurely preserved, but apparently of the type of *L. Veltheimianum* and possibly the same with *L. primævum* of Rogers. The spore-cases are not improbably those of this plant, of the species of *L. gaspianun* which belongs to the same horizon though not found at this locality. The occurrence of this bed is a remarkable evidence of the abundance of lycopodiaceous trees, whose spores must have drifted in immense quantities in the winds to form such a bed. It is to be observed, however, that this is not a bed of coal, but a bituminous shale of brown color, and with pale streak, no doubt accumulated in water and even marine, since it contains *Spirophyton* and shells of *Lingula*. In this it agrees with the Australian Tasmanite which, though composed, in great part, of spore-cases of ferns is, as I am informed by Mr. Selwyn, an aqueous deposit, containing marine shells."

A number of other organic forms are associated with the spores in the black shales. In fact a flora and a fauna of considerable interest are coming to light in these hitherto neglected strata. They have long been known to contain *Lingulas* and *Discinas* and also *Conodonts*, whatever they may be. The bones, plates and teeth of fishes, large and small, have also been found in them, sometimes in great abundance and sometimes though rarely, of surpassing paleontological interest. It is below all of these groups that the forms to which I refer are to be found. Microscopic in size undetermined in character, but invested with both geological and paleontological interest, a considerable harvest awaits the careful student of these hitherto unrecognized forms.

I will not occupy the time of the Association with crude descriptions of such other forms as I have recognized, but leaving this branch of the subject, I will ask you to consider for a few moments, an important office that these minute bodies seem to have rendered and to be still rendering.

The office to which I refer is the production of the great accumulations of petroleum and gas that have been found in the Devonian and sub-carboniferous formations of Pennsylvania, West Virginia and Ohio written the last twenty-five years.

Thanks to the great work carried forward by the second Pennsylvania survey, we are coming to a pretty clear understanding of the geology of petroleum so far as its stratigraphical relations are concerned. This much we know, the great repositories are lenticular masses of sandstone and conglomerate, inclosed within masses of fine-grained shales, sealed up, as we may say, within them, but not quite hermetically. Below these oil sands, heavy deposits of shale, blue, red and black, are known to exist and at still greater depth the Devonian and Silurian limestones are found.

What are the sources of these great accumulations of hydrocarbons that have become in the last quarter of a century an important factor in the commerce of the world? There are but three answers to this question that so far as I know the geologists of our day have to offer. I will state them, but my limits forbid their discussion.

The first answer is that the petroleum and gas with which the oil sands are charged are indigenous to these sand and gravel deposits, formed within them from the decomposition and transformation of land or marine vegetation with which these old beaches have been charged. I scarcely know to whom to credit this

theory, but Professor Lesley refers to it frequently with evident approval.

The second theory is that of Dr. T. Sterry Hunt and is to the effect that the animal matter of the great limestones and notably that of the great Devonian sheet, the Corniferous limestone, is an adequate source of these vast reservoirs of oil and gas. Dr. Hunt has made calculation of the amount of oil that could be furnished by such a bitumen-bearing bed as the Bridgeport limestone of Chicago of Niagara age.

The third theory is that of Newberry. At least, the answer to which I now refer has been presented by him in more detail and defended with more painstaking and ability than by any other, so far as I know. His first extended statement appeared in an article in the Agricultural Report of Ohio for 1869, under the title of "The Rock-Oils of Ohio." I will give some of the main points on which Dr. Newberry relies in establishing the last named theory.

He takes the ground that the great source of petroleum and gas in the field already named, is the bituminous matter of the black shales of which I have been treating. The points that he makes are the following:—

1. The hydro-carbonaceous matter required for such a supply is really present in these shales. Analysis shows them to contain from ten to twenty per centum of organic matter of this character, requiring comparatively little transformation to be converted into the bodies whose origin we are seeking. A ton of these black shales is made to yield in our retorts, from ten to twenty gallons of oil, as a matter of practical business.

2. A line of oil and gas springs marks the outcrop of these formations throughout their whole extent, showing unmistakably that they do give rise everywhere and always to these very products. In other words, the shales are undergoing spontaneous distillation by which these products appear wherever the formations themselves are uncovered.

3. The productive wells of the oil fields actually descend to or near to these shale horizons. When limestones hold oil, as the Corniferous of Canada, Dr. Newberry argues that below them still beds of black shale are to be found as the Utica for example. Wells bored below these Devonian black shales in Pennsylvania, even to the great limestone sheet, are comparatively unproductive. The Corniferous has been struck more than once in the deep wells of Pennsylvania and New York without any large returns.

This view has so much to recommend it that it is not surprising that it has won more general acceptance than any other. In the most complete accounts yet given of the geology of an oil producing district, viz., in the late reports of Carll of the Pennsylvania survey, this theory seems to be adopted as furnishing the best available explanation of the facts involved.

The facts to which I have called the attention of the Association at this time are in harmony with this view and may advance our knowledge by one step of the history of these products, but they do not touch the question in a vital or decisive way. Granted that the black shales are the source of petroleum, I have pointed out the particular element of the shales that is engaged in the work. The really vital fact was already held, in the knowledge that analysis has given us of the composition of the shales. It is their ten per centum of hydro-carbonaceous matter that takes the central place in the argument.

If these various *sporangites*, to adopt Dawson's generic designation, prove to be true sporangia, this fact, as I have said, carries us forward by a step at least in the way of accounting for the origin of petroleum and the other hydrocarbons associated with it. Such bodies, lycopodiaceous spores, for example, as Dawson and Hunt and others have shown, partake of the qualities common to most suberine and epidermal matter. Being extremely rich in hydrogen and carbon they approach resins and fats in composition more nearly than they do woody fibre. Hence they repel water — are not easily moistened and are able to resist decay in a remarkable degree. All of these facts bear upon their contribution to and preservation in the beds of coal and shale of which they form a conspicuous part.

Whatever their value shall be found to be in the production of oil and gas, I have shown by the facts here recorded that the bodies in question are universally distributed throughout the Devonian and sub-carboniferous bituminous shales of Ohio and, by fair inference, through all adjacent territory. Let me recapitulate the facts that bear on this question of distribution.

Dr. Dawson first found these *sporangites* on Kettle Point of Lake Huron, but expressly disclaims knowledge of either their vertical or horizontal distribution beyond the twelve or fifteen feet there exposed. They are next found 1000 feet below the surface in northern Ohio. They are rediscovered in the middle or upper division of the Ohio black shale at Columbus. Then by a more systematic search, as opportunity has offered, they are found in

every portion of the Cleveland-Huron black shale of Ohio, from bottom to top and from Lake Erie to the Ohio river. They are also distributed throughout all the most characteristic portions of the Berea shale. I regret that I have not had opportunity as yet to examine the extension of these strata elsewhere, but it is not at all hazardous to assert that whoever looks for them in the Hamilton black shales of western New York, in the Devonian black shales of Michigan and Indiana and in the southwest extension of the Ohio black shale along the eastern border of the Cincinnati axis across Kentucky and Tennessee, will find them there in the same associations and conditions that have been here described. Dr. Newberry indeed has informed me that a number of years ago he recognized spores in the black shale of Kentucky.

RECENT INVESTIGATIONS AND PALÆONTOLOGICAL DISCOVERIES IN THE WAPPINGER LIMESTONE OF DUTCHESS AND NEIGHBORING COUNTIES, NEW YORK STATE. By WILLIAM B. DWIGHT, of Poughkeepsie, N. Y.

[ABSTRACT.]

THE Wappinger (or Barnegat) limestones of Dutchess and the neighboring counties, New York state, can be properly studied only in connection with the Poughkeepsie slates with which they are closely interstratified. Recent explorations in both the limestones and the slates, but chiefly in the former, have had rich results. These results are important for the following reasons:

(1) The age of these rocks has been hitherto very uncertain. Emmons classed them as Taconic. Mather, with the approval of Professor Hall, called the slates Utica and Hudson River, and the limestones Calciferous and Trenton. In later years, after a review of the region, Sir William Logan and Professor Hall have published the whole belt as "unfossiliferous Quebec."

(2) The question of the age of the Wappinger limestones and the adjoining strata, has an important bearing on that of the existence of the so-called Taconic system. There have been, in recent years, many investigations by eminent geologists into the true nature of the original typical Taconic rocks. On the whole, these have impaired seriously the theory in regard to them, held by

Emmons, if they have not destroyed it entirely, in general scientific opinion. Prof. J. D. Dana, after detailed personal observation, has recently, in the American Journal of Science, contributed weighty, and, as I believe, conclusive evidence of the continuity and identity of the slates and limestones of Dutchess county and their modifications, with the Taconic rocks of Emmons in western Massachusetts and Vermont. Palæontological evidence as to their age would therefore be decisive as to that of the Taconic strata of the above mentioned states.

This evidence, now ample, shows the so-called original Taconic strata to contain the Hudson river, Trenton, Chazy and Calciferous groups, and probably also the Potsdam sandstone. This result harmonizes with palæontological evidence furnished by A. Wing and other observers in the northern extension of the Taconic in Vermont.

(3) While conclusions concerning the Dutchess county rocks have been heretofore founded almost entirely on stratigraphical grounds, since 1878 abundant palæontological evidence has been found. In 1878, Mr. T. N. Dale found many Hudson river fossils in the slates west of the limestone belt. In 1878 and '79 Professor Dana discovered Trenton fossils at Pleasant valley and Rochdale, near Poughkeepsie, in the limestone. In 1879, Mr. R. P. Whitfield discovered the Chazy *Maclurea magna* in the continuation of the Wappinger limestone at Newburgh. In 1879, the writer, by invitation of Professor Dana, accompanied him and assisted in the discovery of the Trenton fossils at the localities above named. Since that date I have continued a most careful exploration of the limestone, with very rich results. I have been able to develop more fully the fossils in the Trenton, and also for the first time to find abundant evidence, by fossils, of the presence of the Calciferous group in extensive deposits. I have been able to trace out to a large extent the mutual relations of the Hudson river slates, and the Trenton and Calciferous limestones. It appears that they are disposed in at least three anticlinal folds, with many local complications. The general strike is about north 30° east (true), and the average dip about 70° easterly.

Partial metamorphism has generally obliterated and always affected the fossils, but a very close scrutiny has revealed a number of interesting fossiliferous localities. Of these, Rochdale,

four miles northeast from Poughkeepsie, is by far the richest. I have found fossiliferous Trenton at different points from Salt Point, ten miles northeast of Poughkeepsie to about eighteen miles to the southeast of that city. Fossiliferous Calciferous appears as far to the north, but none has as yet been found farther south than a point five miles below Poughkeepsie.

The Trenton fossils found by Professor Dana at the time above mentioned were *Orthis tricenaria* and *testudinaria*, *Leptæna sericea*, *Strophomena alternata*, *Orthoceras junceum*, cyathophylloid corals, several crinoid species, and a very minute coral since identified by Professor Dawson as the *Chætetes* (*stromatopora*) *compacta*, previously reported in Canada. Part of an *endoceras*, and a large species of *receptaculites* were also found. A fossil appearing to be *Orthis occidentalis* had also been found by him at Ancram.

My own explorations in the Trenton of the Wappinger valley have developed, in addition to the above fossils, *Orthis pectinella* and *lynx*, *Rhynconella capax*, *Chætetes lycoperdon*, var. *ramosa*, *Petraia corniculum* (probably), *Illœnus crassicauda*, a head and columns of *Echino-encrinites anatiformis*, columns of *Schizocrinus nodosus*, a new species of discinoid brachiopod which I have named *Orbiculoidea conica*, and many fragments of columns of a large *encrinite* of the Canadian "cleiocrinus" type, two centimetres or less transverse diameter. Some of these are too close to *C. magnificus* and *grandis* to be assigned to any other species, though the latter forms are sometimes much larger than the type specimen. Others present considerable variation from these types.

The most remarkable fossils are those which I have discovered in the Calciferous, chiefly at Rochdale. These comprise, of Calciferous fossils of general occurrence in the United States, *Ophileta complanata*, *sordida* and *levata*, and several species of fucoids; of well known Canadian fossils, *Ophileta compacta* and *Triplesia* (*Camerella*) *calcifera*. *Ophileta compacta* has previously been reported from a single locality in the United States, near Middlebury, Vt., by Mr. A. Wing.

There are also a large *holopea* resembling *H. proserpina*, and smaller ones not identified; *pleurotomariæ* resembling *P. Canadensis*, *quebecensis* and *gregaria* of Canada; a minute *ophileta*, new species; a *Murchisonia* resembling *M. gracilis* of the Trenton group; several other gasteropods not identified; one or two species

of quite small and finely striated *orthides;* many undeterminable fragments of *Bathyurus;* also several specimens of *Chœtetes lycoperdon*, var. *ramosa*, well marked. The latter is not, I believe, hitherto reported below the Trenton.

Of *Orthocerata*, I have been surprised to find at least from *twenty-five to thirty species* in this Calciferous rock. Unless *Orthoceras primigenius* may be present, which is doubtful, and excepting *Oncoceras mummiforme* (Whitfield) which appears to be represented, all are new forms in the United States Calciferous. All have close septa, none having wider septa than nine to the inch, among hundreds of specimens collected. Most have from fifteen to thirty-five or more septa to the inch. The species vary in length from under two to twenty-three centimetres, and in breadth from under one to over four centimetres. Curved forms and lateral, mostly marginal siphuncles, predominate. There is also a predominance of large siphuncles of the endoceras type, but generally lacking the internal cone. There is much variety in the shape of the siphuncles and in the taper and shape of the shell. Some of these resemble, and may prove identical with, the Canadian species "Montrealense," "glaucus," and perhaps one or two others, but a large part appear to be new species. Two of these orthoceratites are evidently "endoceras," bringing this genus down to a low horizon.

One of the more common and beautiful species is from one to two inches long, with six to eight annulations, and twenty to forty septa to the inch.

Two species of *lituites* occur here, and a *cyrtoceras* with the marginal siphuncle on the side of the inner curve.

From the above statements it will appear that both the Trenton and Calciferous strata of this region differ greatly in palæontological character from other strata of those ages in the United States, while they resemble the strata of Canada, or of perhaps, some of the localities of the United States immediately bordering on Canada.

It would also appear that the Wappinger limestone, contrary to its received unfossiliferous reputation, is highly "fossiliferous," and that Rochdale, its most interesting locality, is the richest palæontological locality of the Calciferous in the United States, and one of the richest in cephalopodic life of any formation in the country.

The Winooski Marble of Vermont. By G. H. Perkins, of Burlington, Vt.

[ABSTRACT]

The object of this paper was to call attention to the remarkable deposits of variegated marble which extend along the western border of Vermont for about 80 miles, from near Burlington to and beyond St. Albans. Quarries have existed in some of the beds for many years, but recently more extensive operations than formerly have been carried on, and better facilities for the variety and beauty of the marble. The beds are probably of age and form a part of the "Red Sandrock" of western Vermont. The marble is a [illegible] and consists of calcite and dolomite, about forty per cent. of the former to sixty per cent. of the latter, and about the same amount of iron and silica, but in proportions that vary much. It is not granular, but [illegible] some layers, [illegible] and [illegible] portions of the beds are [illegible] but they are very rare. The prevailing color is [illegible] with more or less white [illegible] and green [illegible]. One of the most striking features of the beds are the [illegible] which [illegible] the [illegible] of these [illegible] and green [illegible]. One [illegible] of the [illegible] of the [illegible] about [illegible] from [illegible]. All of these are [illegible]. In some the color is [illegible] from [illegible] the [illegible] is [illegible] and [illegible]

The [illegible] of the [illegible] which is [illegible] that the [illegible] from the [illegible] and [illegible] it [illegible] [illegible] can be taken in the Eastern States. The [illegible] more difficult to [illegible] of the Winooski, or as it is now [illegible] the Winooski [illegible] is [illegible] durability, [illegible] and [illegible] as [illegible] proof of [illegible] and other ornamental work in the [illegible]. Specimens showing the various varieties were exhibited [illegible]

The Great Terminal Moraine across Pennsylvania. By H. Carvill Lewis, of Germantown, Pa.

[ABSTRACT.]

After describing the investigations in New Jersey, and along Long Island and southern Massachusetts, which first demonstrated the existence of a true terminal moraine, and referring to the discovery of similar moraines in Wisconsin and Minnesota, which marked halting places in the retreat of the glacier, the author stated that, having obtained the aid of the Geological survey of Pennsylvania, and, during a portion of his work, the assistance of Prof. G. F. Wright, he had been able to follow and define the southern limit of glaciation for the first time in a continuous line four hundred miles in length across that state, and to find that it was everywhere marked by a remarkable accumulation of glaciated material, which, winding across mountains and valleys, from the lowlands of the Delaware to the great Alleghany plateau, was continuous from end to end, and formed a true terminal moraine to the great ice-sheet that once covered northeastern America.

The method employed in discovering the line of the moraine was to zigzag along its course from the glaciated into the non-glaciated region, and *vice versa*, going each time far enough on the one side to be fully satisfied of the absence of glaciation, and on the other to find undoubted traces of its action.

He then spoke of the marked distinction between the glaciated portion of Pennsylvania and that region south of glacial action. Although the general topography of the two regions is alike, the varied superficial features due to glacial agencies, the far traveled and scratched boulders, the smoothed and striated rock exposures, the unstratified deposit of *till*, the many *kames*, and especially the numerous glacier-scratched fragments and pebbles, — all these deposits are in strong contrast with those south of the moraine, where all the gravels are stratified and the pebbles water-worn, where the rocks are never polished or striated, but, on the other hand, often decomposed to a great depth, and where, except near the seacoast, wide stretches of the more elevated regions are perfectly free from all drift.

Nowhere south of the line of the terminal moraine had the author found any traces of glacial action, all statements by other geolo-

gists to the contrary notwithstanding. When typically developed, the terminal moraine is characterized by peculiar contours of its own. A series of *hummocks*, or low conical hills, alternate with short straight ridges, and enclose shallow basin-shaped depressions, which, like inverted *hummocks* in shape, are known as *kettle holes*. Large boulders are scattered over the surface, and the unstratified *till* which composes the deposit is filled with glacier-scratched boulders and fragments of all sizes and shapes. The average width of the moraine is about one mile.

At many places, however, the limit of glaciation is marked merely by an unusual collection of large transported boulders. This is especially the case in front of a high mountain range, which has "combed out" the drift from the ice.

The general course of the moraine across Pennsylvania was defined as follows: appearing first in Northampton county, a mile below Belvidere, at latitude 40° 49′, it rises through the stratified drift as low gravel hills, which, winding up over the slate hills to the west, are soon developed into an accumulation of typical till, holding kettle holes and filled with boulders. Winding in a great curve first westward and then northward, it reaches the base of the Kittatinny mountain three miles east of the Wind Gap.

Ascending to the top of the Kittatinny mountain, 1600 feet high, the moraine crosses over it, being well shown upon the very summit, and entering Monroe county, crosses the great valley between the Kittatinny and the Pocono, enclosing in its course several moraine lakes. Having crossed this valley, and reached the base of the Pocono escarpment, it swings sharply back and around Pocono Knob, immediately afterwards to ascend the steep face of the mountain to the wide plateau on top, 2100 feet above the sea. Crossing this in a fine curve, and heaped up in an immense accumulation, it goes first north and afterwards west, until it reaches the Carbon county line.

Crossing the centre of Kidder township, Carbon county, it reaches the gorge of the Lehigh river, some ten miles north of Mauch Chunk. It crosses the gorge at Hickory run, and, without swerving from its general northwestern course, ascends mountain range after mountain range in Luzerne county, descends to the valley of the east branch of the Susquehanna, and crosses the river at Beach Haven, here forming immense heaps of drift, afterwards to be washed down the river into terraces.

Then, in Columbia county, following along the base of Huntington or Knob mountain for awhile, it finally ascends it, and crossing over the summit, at a height of 1500 feet above the Susquehanna just below, descends the north slope of the mountain to the broad undulating valley to the north. Taking a northerly course, it follows up on the east bank of Fishing creek to the North or Alleghany mountain. The summit of the Alleghanies in Sullivan county is covered with glacial striæ, and contains boulders and other marks of glaciation. The moraine entering Lycoming county, passes westward along the base of the mountain, crossing in its course the Muncy and Loyalsock creeks, and finally, near the village of Loyalsock, turns at right angles and ascends the mountain.

Having reached the summit of the Alleghanies, over 2000 feet above the sea, it crosses the picturesque cañon of Lycoming creek, and passing west through a wild, wooded region nearly as far as Pine creek, it begins a nearly straight northwestward course, through the southwest corner of Tioga county, and the northwest part of Potter. In the high ground of Potter county, the moraine crosses a great continental watershed, from which the waters flow into the gulf of Mexico, Lake Ontario, and Chesapeake bay. The moraine is here finely shown at an elevation of 2580 feet, being higher than elsewhere in the United States.

The line of the moraine now enters the state of New York, in the southwest corner of Allegany county. Passing still northwest and entering Cattaraugus county, it twice crosses the winding course of the Allegheny river, east and west of Olean; then trending to a point five miles north of Salamanca, in latitude 42° 15′, it forms a remarkable apex, whence to the Ohio line its course is southwest. Turning at right angles to its former course, the moraine passes southwest through the southeast corner of Chautauqua county, and, keeping approximately parallel to the course of the Allegheny river, reënters Pennsylvania, in Pine Grove township, Warren county. It crosses the Conewango river seven miles north of Warren, forming immense accumulations in the valley of the river.

Then trending west in Warren county, still at a general elevation of nearly 2000 feet above the sea, the moraine crosses one gorge after another, and forms a line separating, not only the gla-

ciated from the non-glaciated region, but also the cultivated from the uncultivated and densely wooded region.

In Crawford county, the line appears in the southeast corner, and crosses Oil creek between four and five miles northwest of Titusville.

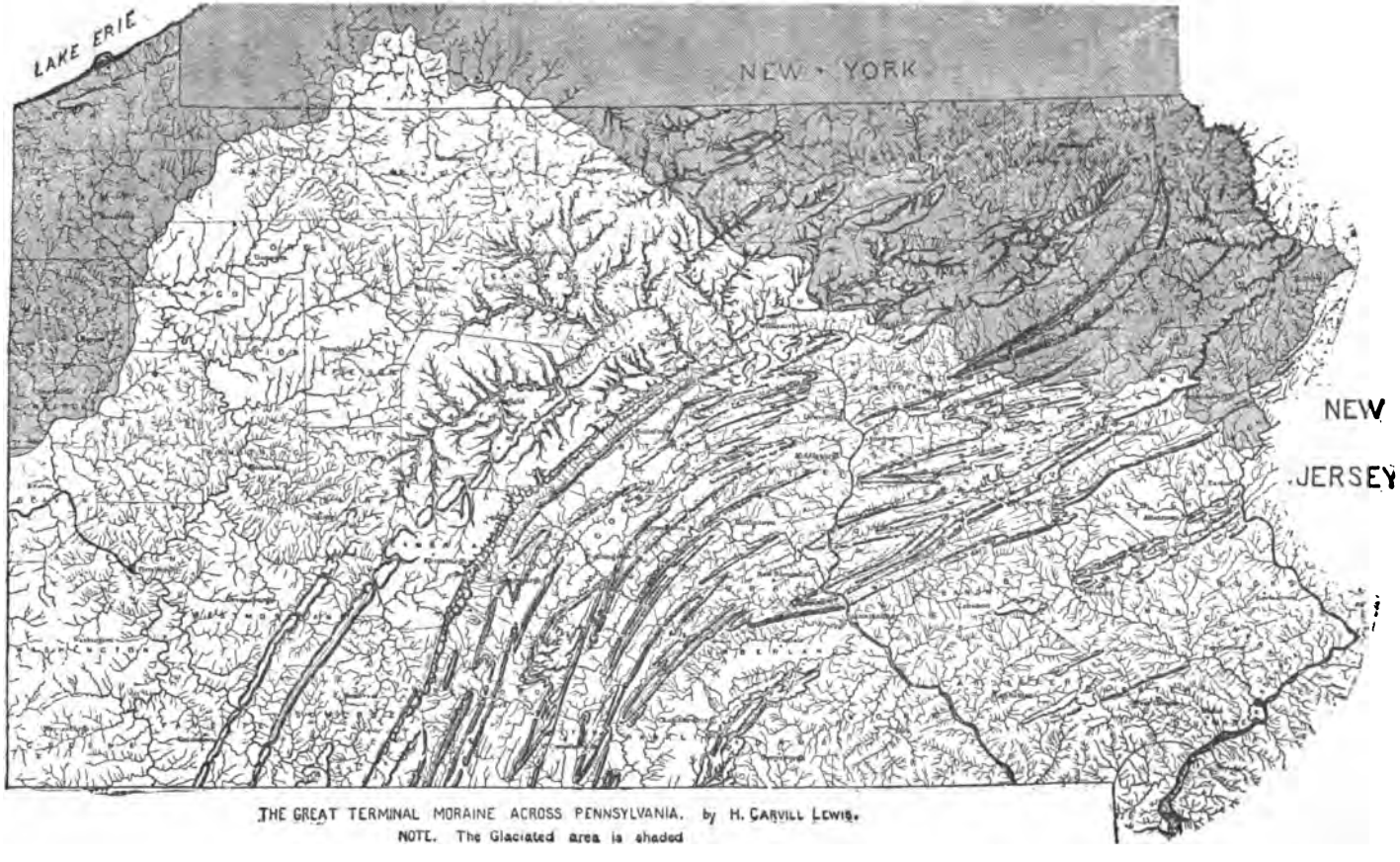
In Venango county it skirts the northwest and west boundary of the county, crossing French creek four miles west of Franklin.

It crosses the three northwest townships of Butler county, and the southeast corner of Lawrence. The Beaver river is crossed by the moraine eight miles south of New Castle.

The moraine traverses the extreme northwest corner of Beaver county, and in the middle of Darlington township, thirteen miles north of the Ohio river, and at a latitude of 40° 50', crosses the Ohio state line.

The moraine thus leaves Pennsylvania at precisely the latitude at which it entered the state, and if a straight line were drawn across the state between these two points, the line of the moraine would form with it a nearly right-angled triangle, whose apex was 110 miles distant perpendicularly from its base. The total length of the moraine, as here shown, is about 400 miles. The moraine crosses the Delaware at an elevation of 250 feet, the Allegheny at an elevation of 1425 feet, and the Beaver at an elevation of 800 feet above the sea, or 225 feet above Lake Erie. Upon the highlands it rises higher by 1,000 feet or more.

Coming to the details of the moraine, the author spoke of the fine development of the moraine in Northampton county, west of Bangor, where it forms a series of hummocky mounds, whose 100 to 200 feet in height, and covered with transported and striated boulders, rise abruptly out of a higher plain to the west. Glacial striæ on the exposed surfaces near Bangor point southwest, or towards the moraine. After following the moraine to the base of the Kittatinny mountain, it became of great interest to know whether a great lobe of ice descended from New Jersey along the lower slope of the mountain, or whether a tongue projected through the Delaware Water Gap, or whether the glacier even so rose to the mountain as to flow over the top of the mountain, unchecked by it and unchanged in its course. The last, the most improbable of these hypotheses, and certainly the least expected by the author, proved to be undoubtedly the true one. The author



THE GREAT TERMINAL MORAINE ACROSS PENNSYLVANIA. by H. CARVILL LEWIS.
NOTE. The Glaciated area is shaded

and the striated surfaces of its enclosed stones, and by its topographical position, it has rarely been subjected to the action of water; (2) that, as proved by numerous glacial striæ and by transported boulders, its course is always at right angles to the direction of glacial movement.

The author stated that the moraine was wonderfully shown upon the summit of Pocono mountain, over 2000 feet above the sea, where a great ridge of moraine hills twelve miles long, one mile wide, and 100 or more feet high, composed of unstratified till, and bearing numerous boulders of Adirondack gneisses and granites, lies upon the level, sandy plain of the Pocono plateau and sweeps around from Pocono Knob into Carbon county. Known locally as "Long Ridge," its origin had never before been suspected. It encloses remarkable little "moraine lakes" without inlet or outlet, and is heaped up into just such conical hills as may be seen in the moraine in southern Massachusetts. Nothing can more clearly show the continuity and uniformity of action of the great glacier than the identity of its moraine accumulations at such remote points.

The author here described the *kames* of Cherry Valley, fine examples of which appear south of Stroudsburg, and are interesting relics of sub-glacial water action. They are composed of stratified water-worn gravel, having often an anticlinal structure, and, as a series of conical hills and reticulated ridges, enclosing "kettle holes," form conspicuous objects in the centre of the valley. They appear to have been formed by sub-glacial rivers, which flowing from the moraine *backwards*, under or at the edge of the ice, emptied into the Delaware valley. He thought that a study of the great subglacial drainage, of which kames are the most prominent relics, would throw much light upon certain high-level stratified gravels whose origin has been ascribed to great changes of elevation.

The author then described the more interesting glacial striæ of Monroe county. One of these, on the north side of the Kittatinny mountain, was six feet wide and seventy feet long. Near the moraine the striæ always curved so as to keep at right angles to it.

He spoke of the very slight effect that the glacier had upon the topography of Pennsylvania. It neither leveled down mountains nor scooped out cañons. The glacier passed bodily across the sharp edge of the Kittatinny mountain without having any appreciable effect upon it, the glaciated part of the ridge being as high

and as sharp as that part south of the moraine. In describing the course of the moraine across Carbon county, he referred to the numerous lakes due to glacial action as being of two kinds:— moraine lakes, or kettle holes in the moraine holding water, and dammed up lakes, due to the obstruction of streams by the unequal distribution of the till. He spoke of the sharp distinction between the glaciated and the non-glaciated region on the Lehigh river, and referred to the striæ upon the summit of Penobscot Knob, which, at an elevation of 2200 feet above the sea point S. 10° W., this being the general direction of the ice flow. On the other hand, in the valley at White Haven, immediately below, the striæ point southeast, or along the valley. He spoke of the importance of separating the high level from the low level striæ, the former indicating the general movement of the top of the ice, which is uniform over large areas, while the latter conform more or less to the valleys and show mere local movements of the base of the ice. The glaciation of Penobscot Knob showed that the ice was at least 2000 feet thick at a point eight miles back of its extreme edge. Data were given, leading to the conclusion that the extreme edge of the glacier had a thickness varying from 400 to 900 feet.

In describing the course of the moraine across Luzerne county, the author showed that it crossed several mountain chains in succession, by each of which it was locally deflected northward.

He stated that at the point where the terminal moraine crosses Buck mountain, in a line diagonally across the mountain, the moraine was so sharply defined that he was able to stand with one foot upon the glaciated and the other upon the non-glaciated region. He described the fine kames between Scranton and Pittston, on the Lackawanna river, and showed that they were nearly parallel to the glacial striæ.

He then gave some details of the course of the moraine as traced through Columbia county. He stated that it was interesting to find that in *front* of a mountain chain, such as Huntington mountain or the Allegheny mountain, the moraine was poorly developed, as though the mountain had *combed out* the drift from the ice.

He described an instructive portion of the moraine, where, three and one-half miles northwest of Berwick, it seems to abut against a high slate hill, which furnishes, therefore, a *section* of the end of the glacier. It shows that the extreme edge of the ice was about 400 feet thick, and that while the moraine and the scratched pebbles

were carried along at the base of the ice, sharp fragments of sandstone were carried on top.

The interesting course of the moraine along the eastern bank of Fishing creek was described, where the glacier stopped abruptly on the downward slope of a hill, stopping simply because its inertia became exhausted. Where the moraine crossed the creek, it formed a great dam, and it was shown that the moraine was often steeper at the back than at its front edge — a fact analogous with the features of modern Swiss moraines.

The evidences of glaciation upon the Allegheny mountain in Sullivan county were given, the striæ pointing south 9° west, and the moraine was followed along the base of the mountain to a point in Lycoming county near Loyalsock, where it climbed up to the great Allegheny plateau, and then, keeping at a high elevation, passed through Tioga and Potter counties into New York.

That this great region of high elevation (over 2500 feet) had a decided influence upon the general course of the moraine was inferred from the local influence already shown by the author to have been exerted by single mountain chains, and he thought it probable that as the lowlands along the Atlantic allowed the ice to flow as far south as Belvidere, so this great mountain region, acting like a wedge, caused the moraine to swing northward into New York; and so, too, the depressions of Lake Erie and the Mississippi valley produced another and more extended southward flow, a portion of which traversed the western part of the state.

Professor Lewis here described the remarkable apex made by the moraine north of Salamanca, N. Y. He showed that it was probable that the Allegheny river flowed under a tongue of the glacier, ten miles broad and two miles long, through a sub-glacial channel at the time of its greatest extension near Olean. He described a great natural dam across the valley of Great Valley creek, near Peth, where the moraine stretches across the valley from side to side, and he spoke of the contrast between the numerous drainage valleys which drained the waters of the melting ice into the Alleghany river, and those valleys which took their rise south of the moraine and were free from all drift.

After giving some details of the western lobe of the ice sheet in Pennsylvania, and referring to its agricultural significance, he spoke of some curious deposits of glaciated material which occurred in a narrow strip of ground immediately in front of the

moraine, and which he had named the *fringe*. These deposits consisted of boulders of Canadian granite, and other rocks, which he found perched upon the summits of hills, sometimes as far as five miles in front of the moraine, though never farther. This glacial "fringe," confined to the western part of the state, was found to increase in width from two miles in Warren county, to five miles on the Ohio line, and was at first a puzzling phenomenon. The hypothesis proposed was that, like breakers on the seashore, the top of the ice overreached the lowest strata by the width of the "fringe," and that while the moraine marked the halting place of the bottom of the ice, by which it was formed, the far-transported boulders were carried on more rapidly in the top strata of the ice, and were dropped outside of the moraine to form the "fringe." It was stated that the striæ in the western part of the state all pointed southeast, being at right angles to those in the eastern part of the state, but like them, pointing always towards the moraine.

In conclusion, the author reviewed the more important facts discovered during his exploration of the line of the moraine, dwelling upon the character of the moraine where crossing river valleys, the absence of proof of any tongues of ice down such valleys, the absence of glacial drift south of the moraine, the very slight erosion caused by the passage of the glacier, and especially upon the deflections, large and small, in the line of the moraine, which were inexplicable on any other hypothesis than that the moraine now described was pushed out at the foot of a continuous ice-sheet of immense extent.

THE SILICIFIED STUMPS OF COLORADO. By ROBT. B. WARDER, of North Bend, Ohio.

[ABSTRACT.]

A GROVE of well preserved Sequoia Stumps at Florissant, Colo., about twenty-five kilometers W. N. W. of the summit of Pike's Peak, was visited by the author in September, 1872. Red granite abounds; and small outcrops of a trap-like rock are numerous.

A small and elevated basin was filled with shales and sandstones, which are still horizontal. Some strata abound in impressions of leaves, similar to the pine, oak, beech, and willow. The prevailing appearance resembles specimens from the Oeningen beds of Switzerland, exhibited in the Museum of Comparative Zoölogy at Cambridge. Some reed-like plants and fish were also found.

Twin Creeks (small tributaries of the South Platte river) have cut through this deposit, to the depth of ten meters or more, exposing the stumps.

The annual rings and medullary rays are well marked. Exposed surfaces are bleached; but many specimens are of a rich chestnut brown, or black, with every appearance of recent wood. The growths of a single year vary much in thickness; the extremes noted were about 1 mm., and 20 or 25 mm. The character of the grain varies much in different trees; sometimes it is straight, sometimes like curled maple, sometimes waved with beautiful regularity. No limbs could be distinguished; but there seemed to be gnarled roots, where the fibres were much confused. In some pieces, the fibres are bent nearly to a right angle, without being broken; in others, the jagged edge looks like brittle wood.

A singular coal-black mass, locally known as the "Carpenter Shop," seemed to be a sort of breccia of portions from 1 to 10 cm. in length, all closely united, with the fibre running in all directions. A few of the stumps are more completely altered, with subvitreous lustre, and red or dull yellow color. One of the least altered specimens lost five per cent of its weight on ignition, including water with a trace of organic matter. Chalcedony and drusy quartz crystals also occur. The former, if in botryoidal or mammillary forms, is commonly called "petrified gum."

These trees, including a specimen five or six meters in diameter, would indicate a milder climate than the present, for frosts are now common as early as September. The endogenous fossil wood found near Denver also supports this conclusion. Hot silicious springs may once have flowed from the volcanic rock; a process of silicification, now in operation on the Yellowstone, is described in Hayden's Report for 1871; but in the Colorado stumps the operation seems to have been far slower and more complete.

The Change of Relative Level of the Ocean and the Uplands on the Eastern Coast of North America. By George H. Cook, of New Brunswick, N. J.

[ABSTRACT.]

At a former meeting of this Association, in this place, in 1857, I read a paper upon this subject. My object in again bringing it forward is to call further attention to it — to the kind of evidence upon which it rests, to the effects of this change, and to the means of ascertaining its various consequences.

1. Submerged forests form the most striking and widespread evidence of this change. William Bartram, in his "Travels through North and South Carolina, Georgia, East and West Florida," etc., in 1791, says: "It seems evident, even to demonstration, that those salt marshes which border the coast of the main, and the reedy and grassy islands and marshes in the rivers, which are now overflowed at every tide, were formerly high swamps of fresh land, affording vast crops of grass, bamboo, a great variety of reeds, and several other trees, the same as are now growing in the fresh swamps whose surface is two feet or more above the spring tides that overflow them. It is plainly to be seen in every part along the coast of [illegible] Georgia, and Florida, to the Mississippi, and I have little doubt but these grassy and marshy [illegible] have been originally of the same kind of vegetable productions, and that they have subsided by the sinking of the earth, and the encroachments of the ocean." [illegible] Mississippi, the United States [illegible] of the coast of Georgia as the Bay of Fundy. As we ascend [illegible] the [illegible] of the [illegible] [illegible] [illegible]

present high tide, and covered by layers of sedimentary matter, on which tall cypresses and other trees are now standing."

On the coast of South Carolina, Professor Tuomey, in his Geology of the State, 1846, records similar facts regarding submerged forests, though he does not think their occurrence indicates any subsidence of the land. He acknowledges that Mr. Lyell has examined the coast of South Carolina and Georgia, and has come to the conclusion that these submerged stumps "show a vertical depression of the land to the amount of at least four feet." In North Carolina Dr. Emmons records the occurrence of submerged forests, in Albemarle Sound. In Virginia, along the shores of Chesapeake Bay, timber is found in the place of its growth, but several feet below the level of tide water, as I am informed by my friend, General R. D. Cutts, of the United States Coast and Geodetic Survey.

The coast of New Jersey is marked by the occurrence of timber and stumps below the present tide level in the marshes which border the State for much of the distance from the head of Delaware Bay to Cape May, and from the last named place to the mouth of the Hudson. In some of these marshes the timber is of white cedar, and has grown in the swamps and soft earth—notably so in the cedar swamps of Dennisville, Cape May County, and in the marsh along the Pennsylvania Railroad between Newark and Jersey City. But there are very many places where oak, maple, gum, yellow pine and other trees of upland growth are found. They are not on the parts of the marsh which are exposed to the action of the sea, but they are still covered by the marsh earth, which must have quietly and gradually advanced and inclosed them, and their stumps and roots are still sound and firm in the solid earth in which they grew. Examples can be seen in the meadows on Delaware Bay, below Salem—in many of the ditches in the marsh near the upland, which have been cut by farmers, everywhere—and a very marked example is to be seen in the yellow pine stumps, in the banks of the canal which connects South River at Washington, N. J., with the Raritan. (Reports Geol. Surv. N. J., and Amer. Jour. Sci., 2d Ser., vol. 24, p. 341.)

On Long Island, in New York, submerged forests occur under the same circumstances as in the marshes farther south. In *The Popular Science Monthly*, vol. 10, p. 434, E. Lewis, jr., records the

occurrence of timber and stumps in the salt marshes at Islip, at Tiana Bay, Peconic Bay, Montauk Point, and Fort Hamilton.

In Massachusetts, Professor Hitchcock, in the Geology of that State, p. 307, under the head of "Submarine Forests," says "they consist of the remains of ancient forests now submerged a few feet below the sea, though sometimes laid bare at low water. . . . . In the Harbor of Nantucket, as I am informed by Lieutenant Jonathan Prescott, portions of cedar, maple, oak and beech trees, some of them in an erect position and accompanied by peat of an imperfect character, were found. All the wood except the cedar, which was nearly as sound as ever, was very much decayed. They were buried by four feet of sand, and were about eight feet below low-water mark.

"Another submarine forest exists at Holmes's Hole, on Martha's Vineyard. It is on the west side of the harbor and was described by the pilot as having the appearance of a marsh at low water. Stumps have been found there in considerable quantity: of the cedar at least. Near the southwest extremity of the Vineyard, on the north shore, I was informed that another forest of a similar description may be seen. On the north side of Cape Cod, also, opposite Yarmouth, cedar stumps may be found (as I was informed by the captain of the Falmouth Packet), extending more than three miles into Barnstable Bay. And Henry Wilder, of Lancaster, who first directed my attention to this subject, says that the same thing occurs in the bay of Provincetown, on the side opposite the village. Further inquiries will no doubt bring to light other instances of a similar character: for my opportunities of observation on this subject have been but few."

In the "Geology of New Hampshire," by Professor C. T. Jackson, p. 250, J. L. Hayes, of Portsmouth, mentions that at Rye Beach "among the curious phenomena which may be witnessed, are the remains of a submerged forest. The stumps and roots of trees of a large size are frequently seen during very low water, on the lower margin of the beach. They appear to have been broken off near the roots, which remain in their original positions. The trees were mostly white cedar. . . . Stumps of trees are found in most of the salt marshes around Portsmouth."

In a paper on the Geology of Portland, Me., by Professor Edward Hitchcock and J. L. Hayes (Boston Journal Natural History,

vol. 1, p. 338), similar circumstances in regard to submerged forests are mentioned.

In the Acadian Geology of Principal Dawson, page 28, is described a submerged forest at the head of the Bay of Fundy, near Fort Lawrence. He says: "At the distance of 326 paces from the abrupt edge of the marsh, and about 25 feet below the level of the highest tides, which here rise in all about 40 feet, I saw the first of the rooted stumps, which appear in a belt of sand, gravel and stones mixed with mud, which intervenes between the slope of mud already mentioned and the level of low tide. Beyond the stump first seen, and extending to a depth of at least thirty to thirty-five feet below the level of high tide, other stumps were irregularly scattered as in an open wood. The lowest stump seen was 135 paces beyond the first; and between it and the water level there was a space of 170 paces without stumps, but with scattered fragments of roots and trunks, which may have belonged to rooted trees broken up and swept away by the ice.

"On digging under and around some of the stumps, they were found to be rooted in a soil having all the characters of forest soil. In one place it was a reddish, sandy loam, like the ordinary upland of Fort Lawrence; in another place it was a black vegetable soil resting on a white sandy subsoil. Immediately over the soil were the remains of a layer of tough bluish clay, with a few vegetable fibres, apparently rootlets of grasses, which seemed to have been the first layer of marsh mud deposited over the upland soil. All the rootlets of the stumps were entire and covered with their bark, and the appearances were perfectly conclusive as to their being in the place of their growth.

"Of thirty or forty stumps which I examined, the greater number were pine (*Pinus strobus*), but a few were beech (*Fagus ferruginea*), and it is worthy of note that these are trees characteristic rather of dry upland than of lower swampy ground. The pine stumps were quite sound, though somewhat softened and discolored at the surface. The beech, on the other hand, though retaining much of the appearance of sound wood in the interior, was quite charred at the surface, and was throughout so soft and brittle that large trunks and roots could be cut through with a spade or broken with a slight blow. Owing to their softness the beech stumps were worn down almost to the level of the mud, while some of the pines projected more than a foot; even these last were, however, much

crushed by the presence of the ice, which, with the tides, must eventually remove them. The largest stump observed was a pine two feet six inches in diameter and showing more than two hundred annual rings of growth. I was informed by respectable and intelligent persons that similar appearances have been observed on the opposite side of the La Planche, and in various other places in the Cumberland Basin. It is only, however, in places where the marsh is being cut away by the current that they can be seen, and the stumps, when laid bare, are soon removed by the ice. Similar beds of stumps and vegetable soil are also occasionally disclosed in digging ditches in the shallower parts of the marshes, and there appears little reason to doubt that the whole of the Cumberland marshes rest on the old upland surfaces."

The west coast of Greenland from about latitude 60° 43 N. to Disco Bay near latitude 69°, appears from various signs and traditions to have been subsiding for the last four centuries. Ancient buildings on low, rocky islands, and on the shore of the main land, have been gradually submerged, and experience has taught the aboriginal Greenlander never to build his hut near the water's edge. The Moravian settlers have been obliged, more than once, to move inland the poles upon which their large boats were set, and the old poles still remain beneath the water as silent witnesses of the change. (Lyell's Principles of Geology.) The evidence in this case, however, is much more like that to be brought forward under the second kind of proofs of change of level.

2. Artificial structures and works of men furnish other evidence regarding this change of level. At Southampton, on Long Island, the first settlement was made by the English in 1640, and the land was laid out in farms and fields before 1650. One of the fields near the seashore and a little west of the village pond was fenced in at that early day. The fence posts must have been set in the meadow soil immediately behind the beach. All traces of this fence were lost sight of till within a few years, the sand beach having been driven inward so as to bury it entirely. But now after long and violent storms, which waste away the sand on the flat or storm beaches, the old posts and the meadows in which they were set are washed clean, and they are seen on the outside of the beaches, and near low-water mark. The meadow soil is still tough and firm, and the tracks of cattle and horses show as

plainly in it as they did when they were made, which was two hundred or more years ago. In that time the sand beach was moved inward its whole breadth or more, and the surface of that time has gone down several feet. The flat sands made by storm-tides are two or three feet above ordinary high water, and the several intelligent gentlemen, residents of Southampton, whom I met, and who had seen them at different times, judged that they were now covered with from four to seven feet of sand. Should another opportunity present, I am in hopes this change of level can be ascertained accurately.

The first settlement near Barnegat, N. J., was made as early as 1690; farms were cultivated on the fertile soils where they first located. They were only a few feet above the level of the salt meadows, which bordered them, and which are always at the level of ordinary high water. The first settlement, which really occupied the best of the soil, has long since been abandoned. The ground is now overgrown with salt grass, and the corn rows in which the fields were last cultivated can just be distinguished. The change of level here cannot be less than two feet, and it may be much more than that.

On the beach at the seaside about five miles below Barnegat inlet, there were settlements made by whale-fishers as early as 1690, and they had houses built, and kept animals on the beach and the adjoining salt meadows. There was also a timber swamp extending along on the inside of the beach for two miles or more. The settlement was long since abandoned, the sand of the beaches has been moved inward several hundred yards, the timber in the swamp is nearly all killed, and the stumps of the trees are some of them still to be seen standing in the flat storm-tide beach. On digging down to find whether they were in their original places of growth, I found them still in the swamp earth, with the bark and fibrous roots attached, and that the surface of the swamp earth was fully three feet below the level of the sand which embedded them, and as low as if not lower than ordinary high water — quite too low for cedar trees to grow now. Two or three persons who have been long familiar with the beach assure me that, for a long distance there, there is salt meadow sod to be seen projecting out on the strand and near low-water mark, — and on this they have seen the tracks of horses, cattle and sheep distinctly marked. These are only exposed after long and violent storms which waste off the

sand and carry it out to sea.[1] And they are again covered with sand when the waves are less powerful and the roll of the ocean brings back the sand and piles it in the flat and steeper-sloped strand of common tides. Tracks would be covered and preserved by the wash of the ocean through glades or low parts of the beach, which would carry forward the sand and deposit it on the surface of the meadow, and the wind finally moves the whole body of the broken beach inward. These circumstances certainly indicate a change of level, and a rise of the sea upon the land within the past two hundred years.

The land on the eastern shore of Delaware bay was taken up, and settlements begun as early as 1650. In the early years succeeding islands of hard and timbered ground in the meadows were cleared and farm buildings erected on them, and the farms were cultivated for many years. Now these islands are covered with water in high tides and only marsh grass is grown upon them. This, too, indicates a vertical rise of the tide-water upon the upland, to the extent of a few feet within two hundred years past.

The consequences of this change in level are to be seen everywhere along the seashore, in its rapid and continuous wearing away by the action of the waves. From Florida to Greenland, as has been mentioned in the former part of this paper, facts recorded show that this is the case. In some instances four hundred or five hundred feet in breadth have been worn from the shore within the last forty years. The wear is so uniform and so consistent too, with the cause, that is, with the change of level, that it does not seem necessary here to cite cases. Another ill effect of the change is that of making the low-lying uplands into tide meadows and marshes. This change is gradual, if they are in wood; the trees die out slowly and no new trees grow — and if it is farm land it becomes less productive from imperfect drainage, the soil is soured, and finally its cultivation is unprofitable. Where the shores are bold these effects are not taken note of at all.

Navigation is improved by this change, the channels must deepen, and the waterways be easier for the passage of vessels — and this to some may be a kind of compensation for the damage which is done to others.

The rapid wear of the shores, and the changes produced by it,

[1] Such a storm occurred in Sept., 1882, and the tracks were found on solid and firm sod, about three feet below mean high-water mark.

have made necessary in many cases the construction of expensive works to resist or divert the increased force of the waves. Sandy Hook has grown longer and narrower gradually for the last hundred years, and jetties have been built out on the sea side of the Hook to stop the wearing away of the land and to afford a lodgment for the sand that is moving with the current. The work appears to be successful. Similar plans were made for preventing the wear about the entrance to Charleston Harbor many years ago. Expensive works are projected to prevent the wear of the shore at Long Branch, and the damages there are so heavy that the protection must soon come. The changes about many of the lighthouses on the low seashores are owing to this cause, and the works for their preservation involve a heavy expense every year. When this change is accepted as a settled truth, and engineers study its consequences and effects, in the places where they are shown, then the proper works for obviating them can be best devised.

It becomes an interesting subject of inquiry as to the cause of this change of level. Facts have not yet been accumulated to a sufficient extent to determine whether this change is local or general throughout the northern hemisphere. At present I am led to look upon it as general. The noted case of subsidence in southern Sweden, which is referred to in all our text books on geology, is of the same kind with those in our own continent.

In Scania, the most southerly part of Sweden, the seaport towns bear evidence that the land is there sinking. Streets originally built above high-water mark are now below it, and even old streets are found at a still lower level, showing that the subsidence has been in progress for a considerable time. A stone, the position of which had been exactly fixed by Linnæus, in 1749, was found in 1836 to be one hundred feet nearer the water's edge than it was eighty-seven years before."—Jukes, p. 332. Lyell, in his "Principles of Geology," gives a long list of cases on the coasts of England, Scotland, France, Holland, and along the Baltic, where the action of the sea is the same as on our coasts, and he gives also numerous cases of submerged forests similar to ours and occurring below the present tide level.

There has been considerable doubt thrown upon the fact of a general subsidence such as is here described, on account of the occurrence of sea-shells and buried timber, both of kinds now living,

in earthy deposits which are a few feet above the present sea-level. Such cases do occur. I have observed them in New Jersey, and they are described by others as occurring in other places along the American sea-coast. But, however recent they may be, they belong to a period which preceded the present one of depression. The wood which is found is none of it so fresh as to be fit for timber or lumber as that of the present depression is, and the occurrence of the shells is more in alluvial sand or mud beds than any that is forming now, and the shells themselves, though of the species still living in the adjacent waters, appear a little different from the recent ones. The present depression may not have been going on more than 500 or 1000 years, and the indications are that there has been another period of depression in which the surface of the upland was ten feet or more nearer the sea-level than it now is. A single instance will illustrate this statement. The Maurice River is a tidal stream which empties into Delaware Bay. About fifteen miles above its mouth the river has cut into its eastern bank so as to expose an ancient oyster-bed, with its mass of undisturbed shells, still in pairs and all in their natural position. This bed is fully eight feet above ordinary high-water mark, and yet there is every indication that it has never undergone any disturbance except in the gradual elevation which brought it above the tide level, and is now lowering it again to its original place beneath the waters.

A careful study of the numerous cases like this will satisfactorily prove that there have been other periods of alternate depression and elevation in comparatively recent times, the phenomena of which are so nearly alike that they are very commonly confounded with each other. And when they are clearly distinguished, it will probably be found that the rise or the depression is one common to our whole coast and probably to the whole northern hemisphere. An accumulation of ice in the northern polar regions would be sufficient to move the centre of gravity of the earth in that direction, and thus to cause an increased amount of water in that hemisphere, and of course disturbance of the relative level of the ocean and uplands, and the apparent subsidence of the land.

On the Classification and Origin of Joint Structures. By W. O. Crosby, of Boston, Mass.

[ABSTRACT.]

This paper is, in a large measure, an outgrowth of the articles by Mr. Gilbert and Prof. LeConte in the American Journal of Science for January, March and July, of the current year. I fully agree with Mr. Gilbert, that parallel jointing must be quite distinct in its origin, as it usually is in its characteristics, from both shrinkage cracks and slaty cleavage. Nevertheless, I cannot follow him in so restricting the definition of joint structure as to exclude shrinkage cracks, chiefly because such a classification appears impracticable. A very large proportion of the planes of division in both stratified and eruptive rocks are due to shrinkage, as is clearly recognized by all in the columnar jointing of basalt, and it is often impossible to distinguish the shrinkage cracks from those produced by other causes. Hence, if Mr. Gilbert's classification were adopted, it would be necessary to coin a new term to be used, in the very numerous cases where the origin of the planes of division can be determined. It seems better, therefore, to class shrinkage cracks as joints, and then to distinguish, where we can, between joints having unlike origins.

Broadening the definition of joint structure in the way here proposed, we are obliged to recognize at least three distinct classes of joints. These are, first, the short, non-parallel and non-intersecting joints dividing the rocks into irregular polygonal blocks and having their origin, as all geologists agree, in the contraction of the rocks, and hence properly called contraction or shrinkage joints; second, the joints near, and parallel with, the surface in granitic rocks, which have been satisfactorily explained by Prof. Shaler, as due to the expansion of the surface under the influence of the sun's heat, and which may, therefore, be appropriately named expansion joints. The third and most important class embraces the straight, parallel and intersecting joints. These have been attributed to shrinkage, compression, magnetism and an unexplained shearing force. The writer concurs with Mr. Gilbert in rejecting all these hypotheses, and advances the idea that earthquakes are the principal cause of parallel jointing.

now and then; and these, it is easy to see, will be parallel, and will intersect without interference, when the strains are varied in direction. But earthquakes account for joints in rocks of all classes and conditions, in the unconsolidated and undisturbed Post-tertiary clays as well as in the ancient, crystalline and contorted gneisses, while the normal accompaniment of compression and plication is cleavage, and not jointing; although neither structure can be developed by these agencies without disturbing the stratification. Earthquakes are contrasted with all the other agencies referred to by their almost instantaneous action; and this peculiarity enables us to explain the remarkably regular jointing of some very coarse and irregular conglomerates; for these rocks break very irregularly, unless the force is applied with great suddenness.

I have elsewhere[1] pointed out that the irregular and ragged outlines usually exhibited by masses of coarsely crystalline eruptive rocks require us to believe that the joint structure does not extend to great depths, but is essentially a superficial feature of the earth's crust. And I have also shown that this view is perfectly consistent with the hypothesis that shrinkage is the chief cause of jointing.

My purpose now is to prove that the argument for the superficial nature of joint structure is strengthened, rather than weakened, when we substitute earthquakes for shrinkage as the principal cause of jointing. It is a well established fact that earthquake-foci are, in the main, comparatively superficial, and also that the rocks composing the earth's crust are much less solid and coherent, that is, much less elastic, near the surface than at great depths. But the number and extent of the fractures produced by an earthquake are inversely proportional to the elasticity of the rocks which it traverses; and just as certainly as the elasticity increases downward, the fractures (joints) must increase upward.

Hence, in conclusion, we may safely say that every theory of the origin of joint structure now entertained by geologists (and in a certain sense they are all true, some explaining one kind of jointing and some another) is consistent with the view that jointing is a superficial structure, the deep-seated rocks being essentially unjointed.

[1]Geological Magazine for Sept., 1881.

The Undulations of the Rock-masses across Central New York State. By Henry S. Williams, of Ithaca, N. Y.

[ABSTRACT.]

Examination of the dip of the rocks exposed in the counties of Chemung, Tioga, Tompkins, Schuyler, Seneca, Cayuga and Ontario has revealed a series of nearly parallel folds trending about twenty to thirty degrees north of east, decreasing in strength from the Pennsylvania line toward the northwest.

A north and south line from Ithaca, N. Y., to Ulster, Pa., shows the following folds:

About five miles south of Cayuga lake is the axis of a syncline, quickly rising into an anticline which crosses the Lehigh valley railroad just south of Newfield Station.

A second syncline crosses the same road between West Danby and Spencer stations, and the U. I. and E. R. R. between Swartwood and Van Ettenville. This seems to be the continuation of the Cowanesque syncline of the Pennsylvania survey, which is seen farther east, at Marathon in Cortland county, N. Y.

Near Van Ettenville there are evidences of a slight syncline in the top of the main anticline. The third syncline crosses the Lehigh valley road between Bingham's and East Waverly. The southern slope of this syncline, with northwesterly dip, appears at Chemung, Chemung Narrows, Waverly and the northern suburbs of Owego.

This is doubtless the continuation of Sherwood's Crooked creek syncline of the Pennsylvania survey.

From Ithaca going northward, the first syncline crosses Cayuga lake near Ludlowville, and Seneca lake at North Hector.

Slight folds are seen along both Seneca and Cayuga lakes farther north, but too slight to determine their axes with precision. The most northern anticline detected is near the foot of Canandaigua lake on the west side.

North of the Pennsylvania line the greatest dip is rarely more than three degrees.

The dips to the northwest are shorter and steeper than those in the opposite direction.

The folds are about twelve miles apart.

Occurrence of Magnetic ore deposits in Victoria County, Ontario. By W. Hamilton Merritt, of Toronto, Canada.

[ABSTRACT.]

In going north from Lindsay, the primary rocks are met with, between Fetty's Bridge and Kinmount.

They here consist of granite, gneiss, syenite and crystalline limestone, with occasional bands of dioritic rocks; the latter are not so strongly developed here as farther east in the Madoc region.

There is a small amount of Labradorite rock or norite, which, however, has been observed at only one place.

Some thirteen deposits of magnetic ore have been located in this Victoria region, the most important of which were visited by me last year; there are numerous other untested occurrences. The ore bodies occur, as is usual with magnetic iron ore deposits, in beds more or less lenticular and of size varying from mere patches to beds of from 40 to 50 ft. in width, and unproved length and depth, enclosed in and running with the country rock.

It is difficult in some cases to discover defined walls, as a layer of rock often comes in between two deposits of ore, or between the main body and a smaller one; and again in other cases there is a complete gradation from the one to the other. Crystalline limestone almost invariably in this district is associated with the magnetic ore deposits, which either occur in it or near its junction, with granitic or hornblendic rocks. Therefore in searching for iron ores in this region, it is well to keep in the vicinity of the bands of crystalline limestone. The ore body at the Paxton mine, though occurring in a syenitic gneiss, is associated with a thin band of crystalline limestone.

Hornblende is very intimately connected with the magnetic ore, and, especially at the above mentioned Paxton mine, one gives way to the other sometimes insensibly and again very abruptly throughout the extent of the workings.

Besides calcite and hornblende, other minerals such as augite, felspar, mica, iron pyrites, quartz and also specimens of olivine and serpentine were found occurring with the ores.

The magnetic ores of this district contain as a rule merely traces of phosphorus, but most of them require calcining to drive

off the sulphur, though some, such as the Paxton ore, are almost entirely free from pyrites. Titanium is as a rule not present in a greater quantity than mere traces, though some deposits such as at Pine Lake carry a notable quantity. The metallic iron in the ore as it comes from the mine averages between 48 and 49 per cent and the selected has about 60 per cent iron.

The beds here, as in other well known localities, occur in defined belts, as for example a belt can be followed from lot 20 in the first concession of Snowdon to lot 30 in the fifth concession and probably farther.

The magnetic ore deposits of the Victoria region considered apart from any others, from their strike and dip with the rocks of the country, their similar crystalline character, their frequent insensible gradation of rock to the ore, and *vice versa*, and the similarity of their intercalated minerals, it would appear inevitable that they had been deposited with the rocks that contained them; either, according to Dr. T. Sterry Hunt, while during the eozoic times chemical activities, dependent upon greater subterranean temperature, different atmospheric conditions and abundance of thermal waters were at work, and therefore we might suppose local deposits of a varied character were going on; or, the neptunean view as ordinarily expounded, with its subsequent metamorphosis.

In the Geological Survey Report for Canada for 1873–74, Dr. Harrington shows how by heat the conversion of a bog ore was effected to a magnetite, having all the characteristics indicated by the name. As deposits of the lake bog ore are taking place at this present day in a manner which would seem to answer all requirements (with subsequent metamorphosis) for the formation of these magnetic ore beds, it seems natural that this theory finds many supporters.

The first developed deposit in this region, known as the Victoria or Old Snowdon mine, occurs in a wide band of crystalline limestone to the east of Kinmount. This ore bed is perhaps the most interesting in Victoria County, owing to its apparent strike, where opened, at an angle of nearly 45° to the general strike of the rocks of the country. It would seem, therefore, in contrast to all other deposits discovered in this region, to have been subsequently deposited in a fissure. The irregularity in the strike of this deposit may, however, when the country can be more thoroughly examined, be found to have been caused by a fault running the length of the

valley, at the edge of which the deposit in question is being worked, for there is evidence of much disturbance where the rocks have been exposed.

The Victoria Railroad runs through this district and as large quantities of timber suitable for the manufacture of charcoal are in the vicinity, there is not a more favorably situated locality for charcoal iron smelting in America, and there will undoubtedly be important developments in this direction.

Comparative View of the successive Palæozoic Floras of Canada. By J. W. Dawson, of Montreal, Canada.

[ABSTRACT.]

In Canada, we possess a very complete series of fossil plants extending from the Upper Silurian age to the Permian, and the object of the paper is to sketch out the characteristics of the several floras and sub-floras which have been studied.

I. *Carboniferous Flora.*

This presents three sub-floras in descending order: (1) That of the *Permo-Carboniferous* or Lower Permian, more especially seen in eastern Nova Scotia. It contains several common Carboniferous species, as *Dadoxylon materiarium*, *Pecopteris arborescens* and *Calamites suckovii*, associated with species peculiar to itself, as *Walchia robusta*, *W. gracilis*, *Calamites gigas* and *Cordaites simplex*. (2) That of the *Coal Formation*, which is the great headquarters of the Carboniferous flora, and is especially rich in *Sigillaria* and its allies, and in ferns. One hundred and thirty-five species of plants have been catalogued from this formation. There are nineteen species of *Sigillaria*, about the same number of *Lepidodendron* and allied genera, fifty ferns and thirteen *calamites* and allied forms. (3) The *Millstone Grit* sub-flora. Here we have a limited number of species, precursors of those of the coal measures, and described in a special report of the author published by the Geological Survey of Canada. *Dadoxylon Acadianum* is a very characteristic conifer of this period. (4) The *Lower Carboniferous* sub-flora. The number of species here is small and many of them peculiar, this formation being separated by

the marine limestones from the Millstone Grit, and on the other hand being unconformable to the Erian or Devonian below. *Dadoxylon antiquius* is a characteristic pine of the period, and *Lepidodendron corrugatum* and *Aneimites Acadica* are very common.

II. *Erian Flora.*

Here we have three sub-floras, all markedly distinct from those of the Carboniferous. (1) The *Upper Erian* sub-flora is characterized more especially by ferns of the genera *Archæopteris* and *Cyclopteris*. *A. Jacksoni*, *A. gaspiensis* and *C. obtusa* are very abundant forms. The flora is similar to that of the Catskill of New York and the Kiltorcan beds in Ireland. (2) The Middle Erian sub-flora, corresponding to that of the Hamilton and Chemung groups in New York, is very rich in species. It contains several coniferous trees of the genus *Dadoxylon* and a great number of beautiful and delicate fern fronds, including sphenopterids and *Hymenophyllites*, as well as such ferns as *Megalopteris*, *Cyclopteris* and *Archæopteris*. *Lepidodendron Gaspianum* is very characteristic, and there are species of *Psilophyton*. *Sigillaria* scarcely appears, but there is an abundance of *Cordaites* (*C. Robbii*). The remarkable aquatic plants of the genus *Ptilophyton* occur here, and there are beds filled with Macrospores or "*Sporangites*." (3) The Lower Erian sub-flora, which is specially characterized by the genera *Prototaxites*, *Arthrostigma* and *Psilophyton*. These plants are often very abundant, but belong to few species.

III. *The Upper Silurian Flora.*

This is limited in Canada to *Prototaxites*, *Psilophyton* and the rounded fruits of the genus *Ætheotesta*, equivalent to *Pachytheca* of Hooker. These occur in rocks attributed to the Lower Helderberg group, and no undoubted land plant has been found in any older formation.

The above mentioned sub-floras can be readily recognized by a competent palæobotanist; and, as the author has shown in reports published by the Geological Survey of Canada,[1] can be correlated with those of the United States and Europe; but any more minute subdivisions are of merely local value.

[1] Report on Erian or Devonian and Upper Silurian Plants of Canada, 1871. Report on Plants of the Lower Carboniferous and Millstone Grit, 1873. Report on Erian or Devonian Plants of Canada, Part II, 1882.

On the Origin of Joint Cracks. By H. F. Walling, of Washington, D. C.

[ABSTRACT.]

Mr. G. K. Gilbert, of the U. S. Geological Survey,[1] has observed in recent and nearly undisturbed clay beds of the former "Lake Bonneville" in Utah, a jointed structure in two directions nearly at right angles with each other. In this case, as he remarks, it is difficult to conceive that powerful compressive forces have been exerted in directions normal to those of the joint planes, thereby producing the jointed structure in a manner similar to that by which slaty cleavage is supposed to have been caused.

Nor does Mr. Gilbert accept the explanation offered by President Le Conte,[2] who, having observed shrinkage cracks in some recent alluvial deposits of California, attributes jointed structure to a similar process of contraction. Shrinkage of strata held by adhesion to their supporting beds, produces a prismatic structure in which the planes of separation are seldom continuous beyond the individual prisms. In jointed structure, on the contrary, the planes of separation are continuous, those of different systems intersecting each other but exhibiting no mutual relations. Regarding the reference to magnetic or to shearing forces for the production of jointed structure as unworthy of consideration, Mr. Gilbert considers the problem an open one.

It is clear that the force which has produced parallel joint cracks must have a definite direction over the wide areas where the parallelism prevails. It seems probable, moreover, that it must have acted with a suddenness, differing from the steady, long continued strains which slowly bend the strata into curved folds. Such a force is supposed by the writer to come into action when sufficiently powerful earthquake waves attend the production of faults.

Assuming that the materials of which the earth's interior are composed are in a sufficiently plastic condition, at least near the surface, to maintain a general equilibrium of pressures, it is evident that when this equilibrium is disturbed by the transfer of

[1] Am. Jour. Sci., Vol. XXIII, June, 1882, and Vol. XXIV, July, 1882.
[2] Am. Jour. Sci., Vol. XXIII, March, 1882.

materials from continents to ocean floors, the inequalities of pressure will tend to move the more plastic substratum which supports the overloaded ocean beds to positions under the lightened continents. The sinking of ocean beds and rising of continents will bend and sometimes fracture the more rigid crust above. Fractures or faults produced in this way will be longitudinal or in the general direction of the axis of denudation, and since the corrugation or folding of strata is normal to the subterranean return flow of plastic material which produces it,[3] by gradually diminishing the width of the belts supporting the upper strata, the axes of denudation and corrugation will be in the same general direction.

The sudden sinking, at the time of fracture, on one side of the fault and rising on the other, originates an earthquake, which is a wave movement outward from the point, or in this case *line*, of disturbance. On and near the surface the wave movement becomes not only a succession of rarefactions and condensations, but a progressive, up and down movement like that of ocean waves. The actual amount of bending in cases of exceptionally powerful earthquakes has sometimes been so great as to cause the ground to visibly open and shut in fissures, sometimes even engulfing and closing in upon men and animals.

Here then we have a sudden wave force, competent to rupture brittle and inextensible materials like rocky strata and clay beds in lines or planes parallel to its origin. Moreover, the sinking on one side and the elevation on the other would be likely, while the originating cause continues in action, to augment from time, especially in the portions of the fault midway between its ends. This would give rise to lateral or transverse bendings and fractures with accompanying transverse waves. In this way two sets of joint cracks nearly at right angles to each other might occur.

[3] See Proceedings Am. Assoc. for the Adv. of Science, St. Louis meeting, 1878, p. 179.

ON THE ASSOCIATION OF CRYSTALS OF QUARTZ AND CALCITE IN PARALLEL POSITION. By R. B. HARE, of Guelph, Ontario, Canada.

[ABSTRACT.]

THE association of crystals of quartz and calcite in parallel position will be illustrated by specimens taken from the Grube "Reicher Trost," Reichenstein, Silesia, Germany. The specimens show the almost perfect similarity of the Reichensteiner and Schneeberger occurrence and defend Eck of Berlin, against von Rath and Frenzel.

TITLES OF OTHER PAPERS READ IN SECTION E.

THE EOZOIC ROCKS OF CENTRAL AND SOUTHERN EUROPE. By T. Sterry Hunt of Montreal, Canada.

THE SERPENTINES OF ITALY. By T. Sterry Hunt of Montreal, Canada.

NOTE UPON THE GENUS PLUMALITES. By James Hall of Albany, N. Y.

ON THE RELATIONS OF DICTYOPHYTON, PHRAGMODICTYUM AND SIMILAR FORMS WITH UPHANTAENIA. By James Hall of Albany, N. Y.

THE FELSYTE-TUFA OF COLORADO. By Alexis A. Julien of New York, N. Y.

THE DUNYTE BEDS OF NORTH CAROLINA.[1] By Alexis A. Julien of New York, N. Y.

THE GENESIS OF THE CRYSTALLINE IRON ORES OF NORTH CAROLINA AND NORTHERN MICHIGAN.[2] By Alexis A. Julien of New York, N. Y.

[1] Published in full in the Proceedings of the Boston Society of Natural History, 1883.

[2] Published in the Proceedings of the Philadelphia Academy of Natural Sciences, 1883.

THE COMPARATIVE STRATIGRAPHY OF THE CRYSTALLINE ROCKS OF NORTH CAROLINA AND CANADA. By Alexis A. Julien of New York, N. Y.

ARCTIC EXPLORATIONS IN NORTH AMERICA. By John Rae of London, England.

THE TOPOGRAPHY AND GEOLOGY OF THE GREAT SALT LAKE VALLEY. By William Bross of Chicago, Ill.

NOTE ON THE FAUNA OF THE CATSKILL RED SANDSTONE. By E. W. Claypole of Yellow Springs, Ohio.

ON AMPHICOELIA CEDARVILLENSIS FROM THE NIAGARA GROUP OF CEDARVILLE, OHIO. By E. W. Claypole of Yellow Springs, Ohio.

NOTE ON THE EXTERIOR MARKINGS OF BARK OF LEPIDODENDRON CHEMUNGENSE. By E. W. Claypole of Yellow Springs, Ohio.

A ROCKING STONE IN NEW YORK CITY. By Chas. Hallett Graham of New York, N. Y.

FRESHWATER LIGNITIC SERIES OF BEDS IN THE CRETACEOUS FORMATION OF FRANCE. By D. W. Kowalevsky of Moscow, Russia.

ON THE SURFACE LIMIT OF THICKNESS OF THE CONTINENTAL GLACIER IN NEW JERSEY AND ADJACENT STATES, WITH NOTES ON GLACIAL PHENOMENA IN THE CATSKILLS. By John C. Smock of New Brunswick, N. J.

SOME MOOTED POINTS IN AMERICAN GEOLOGY. By J. S. Newberry of New York, N. Y.

GENESIS OF NORTH AMERICAN FLORA. By J. S. Newberry of New York, N. Y.

SUGGESTIONS AS TO THE HISTORY OF THE LOWER COAL MEASURES OF OHIO. By Edward Orton of Columbus, Ohio.

THE CAVES OF STAFFA. By F. Cope Whitehouse of New York, N. Y.

SECTION F.

BIOLOGY.

ADDRESS

BY

WILLIAM H. DALL,

VICE PRESIDENT, SECTION F, BIOLOGY.

MEMBERS OF THE SECTION OF BIOLOGY:—

IN compliance with the custom of the Association, your chairman might have made use of this occasion to review the progress for the past year in that department of Biology which is his preferred field of research. Such a review would have had interest chiefly for specialists in Malacology, now, alas! only too few in number, and it has occurred to me that the opportunity might be better utilized in another way. I propose, therefore, to take a brief general review of what has been accomplished in America toward a knowledge of the biology of its mollusks; and, in so doing, to point out certain directions in which the earnest student may find a field ripe for the harvest, and where at present laborers are either wanting altogether or unfortunately few in number. In this way, perhaps I may be able to gain some recruits for this regiment of the scientific army, from among those young and ardent students who have their work, for the most part, still to do, and who have not yet made a choice among the many peaceful and attractive paths which Nature opens to those who would know her and her ways.

The study of mollusks was at first æsthetic rather than scientific, and to this day its popularity is chiefly fostered by the satisfaction of the sense of beauty derived from a contemplation of the exquisite shells produced by some of the members of this branch of the animal kingdom.

An almost purely artificial classification for shell-bearing mollusks was invented by the father of Natural History, Linnæus, which could be mastered by the least scientific mind in a few easy lessons. So convenient did this prove for collectors and others who wished merely to index as easily as possible their cabinets of shells, polished and duly marshalled in pairs, that, when Cuvier and the immortal Lamarck reformed the classification and for the first time created a science of Malacology, opposition was decided and long continued. The early students, seeking to know the relations of animals which were huddled in a few heterogeneous genera by Linné, were the subject of no little opprobrium from the conchologists of that day, and were very generally looked upon as dangerous radicals and unsafe guides. The intercourse between the students of various countries was incomparably less in amount and much more expensive than at present, and various naturalists pursued similar studies unknown to each other, whence arose great confusion in nomenclature which has not yet ceased to trouble the student.

In this condition was the science when Thomas Say, the pioneer conchologist of America, began to publish his studies on North American mollusks. The first scientific article on the shells of this country, published here, was the article on Conchology by Say in the American edition of Nicholson's Encyclopædia printed in 1816.

The determination of species is naturally the first requisite in the investigation of a local fauna, and the work of our earlier naturalists was largely descriptive from the necessities of the case and the state of science at the time. Much had been previously done by foreign naturalists but in an unsystematic way, and for a good many years American students were occupied in the description and iconography of our native mollusks as an inevitable preliminary to the study of geographical distribution and other more philosophical branches of the subject. It is unnecessary here to trace, even did space permit of it, the gradual progress in this direction, to which naturalists in all parts of the country contributed.

At present the molluscan fauna of the land and fresh water shells of North America is remarkably well known, only the older parts of Europe presenting a parallel to it. Something still remains to be done in the southern states, especially in the more

mountainous portions, but as a whole we can hardly expect that the addition of new species will more than equal in number the reduction in our specific lists, which a greater familiarity with already named forms may render necessary.

With marine forms the case is somewhat different. The fauna of Canada and of the eastern coast of the United States north of the Chesapeake has been worked out with great thoroughness by the energy of resident naturalists, among whom it may not be invidious to mention Say, Lesueur, Gould, Conrad, Mighels, C. B. Adams, Stimpson, Couthouy, Dawson, Whiteaves, Smith and Verrill, as especially eminent. On the western coast where a much richer fauna prevails, the labors of Carpenter, Cooper, Stearns, Gabb, Hemphill and others have given us a pretty complete knowledge of the species from California to Sitka. For the region still farther north, in addition to the labors of Middendorf, large material, in part unpublished, has accumulated in my own hands chiefly through investigations made in person and with the assistance of my companions in explorations in Alaska and on the Arctic coast. With the exception of the Alaskan coast the mollusk-fauna of Arctic America is well known, and new species hardly to be hoped for by the most energetic explorer of the litorale.

The exploration of the southern coast, especially that of the Gulf of California, is much less thorough and indeed from Georgia round the peninsula of Florida to Galveston, there is hardly any locality which would not repay a careful search. Nearly every such attempt results in new discoveries. The question is somewhat complicated by the inchoate state of our knowledge of the West Indies, whence multitudes of marine shells have been described, but in a scattered way and often without (or with an erroneous) statement of habitat. Our knowledge here has not progressed far enough to permit of tenable deductions in regard to geographical distribution and a wide field is open for research.

The study of geographical distribution is one of the most fascinating, far-reaching and important branches of biology. By its help problems of world-wide significance find solution; but it requires a thorough knowledge of the forms involved, a uniformity in the use of nomenclature, and some acquaintance with the palæontology of the later tertiary strata. Without these,

or sound and to distinguish it from other calls or sounds of a similar character; and, since that publication appeared, he has received several less exact communications which would seem on the whole to confirm this conclusion.

We can hardly doubt, however (whether the particular instances referred to afford proof of it or not), that in a group containing so many animals of a high degree of organization, such as the cuttles and squids, for instance, mental processes of a tolerably complex nature must be carried on in many cases. That this subject has received no attention is a matter of regret and it would seem that a mere mention of it would be sufficient to set some observer at work. Given an aquarium or a snailery, both easily and cheaply constructed and stocked, and patience, time and a little ingenuity will supply all the needed conditions. It would not only seem to be a kind of research peculiarly adapted to attract and amuse an invalid, but one which, if properly conducted, would afford results of the highest scientific interest and value.

There is still another branch of the subject, of which the methods applied to other forms of animal life have occupied a very large portion of the thought and activity of the scientific world for more than twenty years. The results have revolutionized science and justly immortalized the remarkable naturalist who led the way. I refer to the modification of organic life by physical causes and the perpetuation of modifications by natural and sexual selection.

The fact does not seem to have been dwelt upon that the action of natural and especially of sexual selection is much more pronounced, and its effects necessarily must be far more important among organisms either in themselves of a high physical and mental rank reacting on each other (as birds and mammals), or with those upon which the actions of highly organized beings are effective in bringing about a selection of characters as in the case of plants visited by bees and wasps.

It must be granted that natural selection is universal in its action, and self-evident in its truth. It does not therefore follow, however, that it is equally efficacious in perpetuating peculiarities among all kinds of organisms. Indeed on consideration it will appear clearly that it cannot be, and that the more simple and plastic the organism, the less likely it is that natural selection will succeed in confining its variations within any given lines. Take, for instance, a pond full of *Amœba*, millions in number. The probability that

any given proliferation, or set of proliferations of the uniform protoplasm of any individual or number of individuals, can be so beneficial as to have any perceptible effect on subsequent generations, is mathematically infinitesimal. And this which is so evident in the case of structureless *Amœbæ* is more or less true throughout all the lower forms of life. It is only when advanced to a comparatively high stage of differentiation that organisms can offer, as it were, a handle for natural selection to take hold of.

There is probably no change or special characteristic in any organism of sufficiently high rank for which a plausible explanation cannot be assumed by the aid of natural selection. Herein lies the danger of this hypothesis. The offered explanation always *may* be true, but students have not always remembered that it also always *may* be false, and that therefore the hypothesis only becomes scientific when it is supported by proof. The *onus probandi* is not lessened by the production of a plausible hypothesis. The proof is often forthcoming in the case of higher organisms, and found with comparative ease. The difficulty increases with each step lower in the scale and practically, as I have shown, becomes insuperable among the lowest organisms. But it is among the lowest organisms that we must look for the first steps toward the grand divisions in which organic life is marshalled. It is here that the key to the great problem of systematic diversity in life is to be found, if at all, and it must be confessed, even at the cost of some disappointment, that natural selection alone does not meet the needs of the case.

American naturalists have from the first recognized more or less clearly the limitations of Natural Selection. By none has it been more clearly stated that natural selection originates nothing, and merely discriminates between already existing differences. By no other geographical group of students have such important expositions been made of the effect of physical forces on the organism and its parts, the relations of anatomical structure to strains and impacts, and of what may be termed the *rhythm* of development as shown by periodic acceleration and retardation in the life of the individual, of the group and of the race. The labors of Cope, Hyatt, Ryder, Morse and others among us, are no less fruitful than suggestive in these directions.

Some unnecessary anxiety seems to have been expressed by many in regard to the origin of variation. It is not doubtful that

variation is as much a necessity as natural selection. In all the unnumbered organisms which have existed since the appearance of life on the globe there can have been no two individuals whose circumstances, as regards their internal contents and qualities or their environment, have been absolutely identical. Many approached nearly to it, but absolute identity has not existed. It is therefore not a matter of surprise that variations exist, but rather that there is anything like uniformity among organic beings. The thing that is really extraordinary is that organic life should have so run in ruts, as one might say, that we can, in the majority of cases, refer without doubt a given organism to one of a few great fundamental groups which can be counted on the fingers.

I feel a reasonable confidence that the solution of the mystery lies in the direct physical action of the environment, by ways and methods few of which are yet understood, many of which may long remain mysterious but which in the course of time will undoubtedly be made clear by the research, the energy, the perseverance and the will of that most extraordinary of all organic mysteries, the human mind.

In this connection it seems a duty to refer to that remarkable work, the Principles of Biology, by Herbert Spencer, in which the actions and reactions of organism and environment are so clearly set forth, so many lines of investigation indicated and such pregnant suggestions offered to the philosophical inquirer, that we may fairly regard it as foreshadowing a major part, not only of conclusions definitely arrived at since its publication, but of those which are still hovering in the limbo of probability.

Since the mollusks occupy, in a certain sense, a middle ground between the higher groups, where natural selection has its freest play, and those lower categories, where its operations are veiled or inefficient, investigation of the laws of variation and of those influences which tend to stability of characters, is among them especially desirable.

We find, as by hypothesis we should find, that the influences of selection are most evident in those mollusks which by their terrestrial habitat are brought into the closest contact with enemies of relatively high intelligence such as birds and other vertebrates. In these the selected characteristics are chiefly those of color. The gray and dull appearance of species inhabiting arid regions is well known. It is not impossible that the primary cause of

this may be due to a less fluent secretion of the animal products which are the chief components of the pellucid substance and glistening epidermis of shells native to moister regions. It would naturally occur in the dry air of the desert that fluid products unprotected by an impervious epidermis would be less readily retained within the soft tissues of snails. In moister regions a tendency is especially noticeable among operculated Pulmonates, to striped markings of darker and lighter color, such as would tend to conceal them among the lights and shades of grass and herbage which they frequent; and one is tempted to refer the perpetuation and wide prevalence of this style of coloration to causes similar to those by which some have accounted for the vertical stripes of the tiger.

But to what is the origin of the microscopic sculpture of the same shells' most delicate and intricate tracery, constant in the species, to be attributed? It does not seem possible that it can be in any way useful.

Again, in tropical species the reflected lip of the aperture is often painted with exquisite rose, orange or yellow hues, entirely invisible under ordinary circumstances when the mollusks are alive. As they are reciprocally hermaphrodite, sexual selection can have no effect in fixing tints of beauty here. In fact, throughout the group, the most puzzling question, and one which presents itself continually, is that which demands the origin of complex combinations of color and form to which it is impossible to assign any utility. There is an Asiatic species of *Bulimus* of a lovely lemon color which, about the beginning of its last turn before forming the reflected lip which marks maturity, deposits a narrow black or bluish band across the whorl. There is no ridge, no interruption of the smoothness of the shell, no internal process corresponding to the band, nor any other touch of such color elsewhere.

An artist would say at once, the lemon color is heightened and the beauty of the shell greatly enhanced by its contrast with the dark band. But our mollusk is no æsthete and his poor eyes are believed to barely help him to distinguish light and shade, certainly not form or color. Yet we cannot doubt that there is a good and sufficient cause for this and numberless similar features among land shells.

Among marine forms the struggle for existence, after the embryonic stages are past, is much less violent. This arises partly from

the much more uniform conditions of life in the sea, partly from the general abundance of food contained in the sea water, and partly from the less intelligence of enemies, which are chiefly fishes and predaceous mollusks.

Since here development of superficial characters in any direction is hardly governed or limited by any sort of selection, we have most remarkable variety exhibited in externals with marked uniformity in structural features. Here, again, we are met with the question, What purpose is served by all this infinite variety of detail which is often, without a microscope, invisible to the observer, and how much more so to the creature itself and its companions of every kind? The exquisite painting of many species of *Conus* is visible only after death, being covered during life with a dense epidermis. The exceedingly delicate ramifications of many Murices, the shagreening of many Trochoids, the richly painted apertures of *Phyllonotus* and the lovely domes of the Cowries,—all these are beyond the perception of their owners or fellow creatures, are utterly useless to them as far as we know, and yet are stable characters within the species.

In certain features Hyatt has traced the influence of gravitation. Food has, without question, great importance especially in determining certain tints of color. I have shown how spirality might have developed from the effect of physical causes seconded by natural selection. In Alaska I noticed a species of *Litorina* which, when living on isolated rocks subjected to the full power of the surf, exhibited modification. The spire became shorter and nearly flat. The last whorl and aperture became enlarged and patulous; strong revolving ridges became rows of rounded nodules and the axis of the shell broad and thick. From this I concluded that those specimens which tended to vary in the directions mentioned possessed advantages over the ordinary form in resisting the action of the waves and therefore retaining their hold upon the rock and life. Once washed into the shifting sands between the rocks, they had small chance of survival. I have often thought of this as one of the most obvious instances I have observed of the action of selection among marine mollusca.

The Carrier-trochus family (*Phoridæ*) are remarkable examples at once of the unexplained acquisition of a valuable habit, its perpetuation by natural selection and in some cases its continued retention when no longer useful.

Certain of these shells pick up and attach to their upper surfaces bits of dead coral, stones and fragments of shell until they are entirely covered. Viewed from above as they must be for the most part by fishes and carnivorous mollusks nothing but dead and refuse matter is visible. It is evident that this must be a great protection both from enemies who would pass them by and from accidental concussions. It may be due to the latter incidental protection that the shells are or have become very thin and indeed barely more than a layer of cement sufficient to hold this extraneous matter in place. It is difficult to imagine how such a habit can have originated, for it is not the practice of gastropods in general and it must have been pretty generally adopted before it could have been of sufficient benefit to the genus at large to have been confirmed as a habit by selection. When this had once taken place its usefulness is obvious. However, it is still more strange that so valuable a habit should, as it has done in many species, fall partially into disuse, so that while the disadvantage of carrying round a burden of projecting and obstructive fragments is retained and indeed sometimes exaggerated by placing the fragments on the extreme periphery of the carrier's shell, the animal should fail to sufficiently cover itself so as to make its load a real protection. This is the case in several species which moreover exhibit a decided discrimination in the materials they collect, so that one species picks up only small dead bivalves, another fragments of coral, and, in one or two cases, only minute pebbles or grains of sand which are of no protection whatever and present the appearance of a mere attempt at decoration. The character and disposition of the load have become so uniform in many cases as almost to take rank as specific characters.

The few instances I have mentioned are merely typical of myriads. They sufficiently indicate the world of mystery which surrounds the life and progress of the members of an entire subkingdom of animals. General readers, perhaps some students, often gain an impression from the steady stream of scientific literature and the enthusiastic claims of some disciples of nature, that we are rapidly illuminating the inmost recesses of the world of knowledge,—that in fact there is little left to explore.

But those whose high privilege it has been to commune with Nature in her sacred haunts, to waste in studious mood the midnight oil and grasp by efforts slow, severe, prolonged, some new truth

sound conclusions cannot be arrived at; and hypotheses, like inverted pyramids, are in perpetual danger of complete overthrow .

In this connection I may refer to a field in which no one is systematically at work and which seems to me to promise rich reward to patient and thorough investigation. Within a comparatively short period many lake basins and fresh water deposits have been explored for mollusks recent and fossil. Among these may be mentioned sundry Dalmatian lakes, Lake Baikal, Lake Tanganyika, the lakes of Nicaragua, the Colorado basin and the Laramie beds of our western territories. I have long wondered at the curious repetitions of form and ornamentation which occur in certain *Unionidæ* or fresh water mussels from Burmah, Pegu, Lake Nicaragua, our western Tertiary beds, and last of all are now reported from Tanganyika. *Camptoceras*, originally described from a living species in India, is now reported fossil in England. *Melanopsis*, now not living nearer than Spain, has lately been discovered by White fossil in the Laramie beds; and, as in New Zealand, associated with a slipper-limpet which appears to belong to the genus *Latia*. *Pyrgulifera* Meek, from the Western Tertiaries, offers no characters by which it may be distinguished from Smith's Tanganyika *Paramelania* more recently described. Several forms described by Neumayr and others, from ancient European lake basins, seem nearly identical with American fluviatile genera. In spite of the opinion of the eminent French editors of the Journal de Conchyliologie (biassed a little I fear by the enormous distance intervening), I can see no valid characters by which the shells of a whole group of living mollusks (*Baikalia*) from Lake Baikal can be distinguished from our fossil *Tryoniæ* of the Colorado basin.

These facts, if confirmed as I think they would be by more thorough comparison and research, must have a deep significance. Who will devote himself to the solution of the problem?

Another line of investigation, closely connected with the geographical distribution of species, has recently been taken up with rich results. I refer to the exploration of the bed of the deep sea. Its relations are American rather from the fact that its initiation began under the auspices of the Coast Survey of the United States, and that its latest methods are largely due to the inventiveness of American naval and Fish Commission officers, than from the results of the work itself, which are world-wide in

their application, and to which various nations, especially the English, have contributed in no stinted measure.

It was thought when deep sea dredging was in its infancy that the unexplored depths, especially in tropical waters, might shelter lingering representatives of distant geological epochs, and that ammonites and trilobites might put in an appearance when search was made in proper quarters. Indeed, some naturalists went so far as to assert that the fauna of the deep sea was essentially that of the cretaceous period which had existed there uninterruptedly from past ages. These ideas have been swept away by actual research and while we cannot be said to know more than a small proportion of the abyssal fauna, we know enough of it to be sure that it is essentially of a recent character. The survival of some forms which once were supposed to exist only in a fossil state does not materially alter the modern character of the assemblage as a whole.

The knowledge of the conditions which exist over a great part of the sea-bed, and of the topographical features of the bottom, has however greatly modified the accepted views of geographical distribution. We now know that the conditions are such that there is no evident reason why a deep-sea species might not extend its range from the Antarctic Sea to the North Polar regions, and in fact very nearly such a range has been made out for a few forms. The free access of Antarctic cold waters to the northern ocean areas has even led to the suggestion that the Antarctic is the great centre of distribution from which species of all shores may have originated. However true this may be of some branches of the animal kingdom, our knowledge of the mollusca hardly warrants so general a deduction. A certain portion of our fauna, however, may be thus derived.

We know that at a relatively not very distant geological period the conditions of life on the globe were much more uniform than now. Both marine fossils and vegetable remains in the boreal and Arctic regions show that a much milder climate once prevailed there, while if we assume a relative disparity as great as now exists between arctic and tropical conditions at that time, we should have a heat in equatorial regions fatal to most life. Hence it is certain that a greater uniformity in conditions and in faunal characters once prevailed, and we cannot but believe that our present fauna is the descendant with modification of that ancient one. Certain

factors have so influenced the climate of the globe that that uniformity has been lost. Yet it would seem probable that the inhabitants of different coasts mainly originated not far from where we now find them, though important exceptions to this rule doubtless exist. The region which has in later times exhibited the most striking contrast of conditions of climate and sea level to the former general uniformity is the Arctic. It is geologically inferrible that this change has been, not slow and constant in one direction, but more or less fluctuating. The Arctic mollusks lend probability to this view for they are almost without exception characterized by wide specific variability; and the genera to which they belong are known as particularly variable even in those species which are not Arctic in their habitat. As the change in environment acted on the ancient fauna, one effect would be to exterminate such species as least readily accommodated themselves to changed conditions and to preserve a fauna of rapid and facile adaptability; perhaps mingled with a few forms particularly indifferent to influences of environment. These last must however in a large fauna always be exceptional.

This is the actual character of the Arctic mollusk-fauna and such features are only less remarkable in that of the Antarctic, because the latter region is almost destitute of coasts and in deep water such fluctuations of climate and sea level as have been referred to would be less readily transmitted and would lose most of their effect.

Other reasons why we may suppose that the present coast faunæ originated in the vicinity of their habitat are afforded by the results of a study I was enabled to make of the relations between the litoral and the immediately adjacent abyssal fauna in the West Indies from the collections of the Coast Survey steamer Blake, made under the direction of Prof. A. Agassiz.

These showed that more than ten per cent of the deep water species were also common to the litorale while a third of the genera were common to both shore and adjacent sea-bed. Only about one and a half per cent were distinctively arctic forms, such as we might expect to be in a large majority were the fauna derived from the North or South Polar Seas exclusively. In the same way we may explain the occurrence of *Voluta* and *Nacella* in the Aleutian Islands, rather as a survival from the Crag period when *Voluta Lamberti* flourished in Britain, than as offshoots from the Pat-

agonian fauna which have travelled northward and left no progeny by the way.

These somewhat crude and perhaps premature generalizations indicate the nature of some of the problems of which a study of abyssal life may afford a solution.

Studies of the embryological stages of a few of our American mollusks have been made in this country and creditable contributions to this branch of the subject have appeared almost every year for several years. Of the total number of American mollusks, those of which we have even an approximate embryological knowledge may be counted on the fingers, and the field therefore is vast. Special training and indefatigable patience, as well as facilities not at the command of all, are absolutely requisite for this most delicate form of investigation and we can hardly hope for rapid progress. Still the results of such enquiries, when properly authenticated and confirmed, are of such exceptional value as to be well worthy the devotion they require.

The anatomical features of the adult mollusks except in a few groups have been much neglected and are of the first importance. Binney and Leidy have done much among the land and fresh water species, and our Pacific Nudibranchs have been the subject of an important monograph by Bergh, while sundry species of marine mollusks have been more or less completely studied by various authors. It is nevertheless true that the soft parts of our commonest species are mostly entirely unknown or but superficially studied, so that even the characteristics of the adult anatomy of our common oyster are differently described by different naturalists and have been monographed by none. This is a reproach that it is in our power to remove, as the facilities required are comparatively limited and inexpensive and few students are so situated that mollusks of one kind or another are not within easy reach. It is true that Americans are not alone in remissness in this direction but that is no sufficient excuse.

The instinctive or mental phenomena which may be exhibited by living mollusks have not been subjected to investigation. It is true that a few observers have incidentally described actions which on mature consideration might be held to exhibit mental processes of a certain kind. The speaker has lately put on record certain observations which seem to indicate the possession, by some species of the genus *Helix*, of the power to recognize a call

or sound and to distinguish it from other calls or sounds of a similar character; and, since that publication appeared, he has received several less exact communications which would seem on the whole to confirm this conclusion.

We can hardly doubt, however (whether the particular instances referred to afford proof of it or not), that in a group containing so many animals of a high degree of organization, such as the cuttles and squids, for instance, mental processes of a tolerably complex nature must be carried on in many cases. That this subject has received no attention is a matter of regret and it would seem that a mere mention of it would be sufficient to set some observer at work. Given an aquarium or a snailery, both easily and cheaply constructed and stocked, and patience, time and a little ingenuity will supply all the needed conditions. It would not only seem to be a kind of research peculiarly adapted to attract and amuse an invalid, but one which, if properly conducted, would afford results of the highest scientific interest and value.

There is still another branch of the subject, of which the methods applied to other forms of animal life have occupied a very large portion of the thought and activity of the scientific world for more than twenty years. The results have revolutionized science and justly immortalized the remarkable naturalist who led the way. I refer to the modification of organic life by physical causes and the perpetuation of modifications by natural and sexual selection.

The fact does not seem to have been dwelt upon that the action of natural and especially of sexual selection is much more pronounced, and its effects necessarily must be far more important among organisms either in themselves of a high physical and mental rank reacting on each other (as birds and mammals), or with those upon which the actions of highly organized beings are effective in bringing about a selection of characters as in the case of plants visited by bees and wasps.

It must be granted that natural selection is universal in its action, and self-evident in its truth. It does not therefore follow, however, that it is equally efficacious in perpetuating peculiarities among all kinds of organisms. Indeed on consideration it will appear clearly that it cannot be, and that the more simple and plastic the organism, the less likely it is that natural selection will succeed in confining its variations within any given lines. Take, for instance, a pond full of *Amœba*, millions in number. The probability that

any given proliferation, or set of proliferations of the uniform protoplasm of any individual or number of individuals, can be so beneficial as to have any perceptible effect on subsequent generations, is mathematically infinitesimal. And this which is so evident in the case of structureless *Amœbæ* is more or less true throughout all the lower forms of life. It is only when advanced to a comparatively high stage of differentiation that organisms can offer, as it were, a handle for natural selection to take hold of.

There is probably no change or special characteristic in any organism of sufficiently high rank for which a plausible explanation cannot be assumed by the aid of natural selection. Herein lies the danger of this hypothesis. The offered explanation always *may* be true, but students have not always remembered that it also always *may* be false, and that therefore the hypothesis only becomes scientific when it is supported by proof. The *onus probandi* is not lessened by the production of a plausible hypothesis. The proof is often forthcoming in the case of higher organisms, and found with comparative ease. The difficulty increases with each step lower in the scale and practically, as I have shown, becomes insuperable among the lowest organisms. But it is among the lowest organisms that we must look for the first steps toward the grand divisions in which organic life is marshalled. It is here that the key to the great problem of systematic diversity in life is to be found, if at all, and it must be confessed, even at the cost of some disappointment, that natural selection alone does not meet the needs of the case.

American naturalists have from the first recognized more or less clearly the limitations of Natural Selection. By none has it been more clearly stated that natural selection originates nothing, and merely discriminates between already existing differences. By no other geographical group of students have such important expositions been made of the effect of physical forces on the organism and its parts, the relations of anatomical structure to strains and impacts, and of what may be termed the *rhythm* of development as shown by periodic acceleration and retardation in the life of the individual, of the group and of the race. The labors of Cope, Hyatt, Ryder, Morse and others among us, are no less fruitful than suggestive in these directions.

Some unnecessary anxiety seems to have been expressed by many in regard to the origin of variation. It is not doubtful that

variation is as much a necessity as natural selection. In all the unnumbered organisms which have existed since the appearance of life on the globe there can have been no two individuals whose circumstances, as regards their internal contents and qualities or their environment, have been absolutely identical. Many approached nearly to it, but absolute identity has not existed. It is therefore not a matter of surprise that variations exist, but rather that there is anything like uniformity among organic beings. The thing that is really extraordinary is that organic life should have so run in ruts, as one might say, that we can, in the majority of cases, refer without doubt a given organism to one of a few great fundamental groups which can be counted on the fingers.

I feel a reasonable confidence that the solution of the mystery lies in the direct physical action of the environment, by ways and methods few of which are yet understood, many of which may long remain mysterious but which in the course of time will undoubtedly be made clear by the research, the energy, the perseverance and the will of that most extraordinary of all organic mysteries, the human mind.

In this connection it seems a duty to refer to that remarkable work, the Principles of Biology, by Herbert Spencer, in which the actions and reactions of organism and environment are so clearly set forth, so many lines of investigation indicated and such pregnant suggestions offered to the philosophical inquirer, that we may fairly regard it as foreshadowing a major part, not only of conclusions definitely arrived at since its publication, but of those which are still hovering in the limbo of probability.

Since the mollusks occupy, in a certain sense, a middle ground between the higher groups, where natural selection has its freest play, and those lower categories, where its operations are veiled or inefficient, investigation of the laws of variation and of those influences which tend to stability of characters, is among them especially desirable.

We find, as by hypothesis we should find, that the influences of selection are most evident in those mollusks which by their terrestrial habitat are brought into the closest contact with enemies of relatively high intelligence such as birds and other vertebrates. In these the selected characteristics are chiefly those of color. The gray and dull appearance of species inhabiting arid regions is well known. It is not impossible that the primary cause of

this may be due to a less fluent secretion of the animal products which are the chief components of the pellucid substance and glistening epidermis of shells native to moister regions. It would naturally occur in the dry air of the desert that fluid products unprotected by an impervious epidermis would be less readily retained within the soft tissues of snails. In moister regions a tendency is especially noticeable among operculated Pulmonates, to striped markings of darker and lighter color, such as would tend to conceal them among the lights and shades of grass and herbage which they frequent; and one is tempted to refer the perpetuation and wide prevalence of this style of coloration to causes similar to those by which some have accounted for the vertical stripes of the tiger.

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the much more uniform conditions of life in the sea, partly from the general abundance of food contained in the sea water, and partly from the less intelligence of enemies, which are chiefly fishes and predaceous mollusks.

Since here development of superficial characters in any direction is hardly governed or limited by any sort of selection, we have most remarkable variety exhibited in externals with marked uniformity in structural features. Here, again, we are met with the question, What purpose is served by all this infinite variety of detail which is often, without a microscope, invisible to the observer, and how much more so to the creature itself and its companions of every kind? The exquisite painting of many species of *Conus* is visible only after death, being covered during life with a dense epidermis. The exceedingly delicate ramifications of many Murices, the shagreening of many Trochoids, the richly painted apertures of *Pleurotoma* and the lovely domes of the Cowries,—all these are beyond the perception of their owners or fellow creatures, are utterly useless to them as far as we know, and yet are stable characters within the species.

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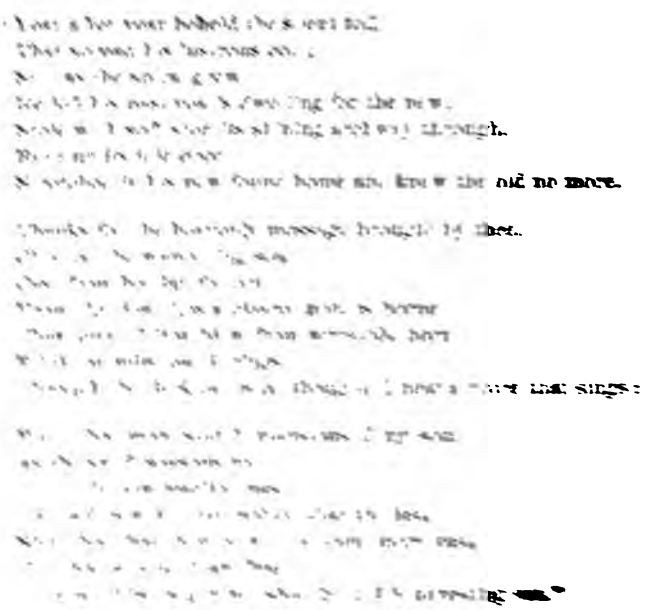
But those whose high privilege it has been to commune with Nature in her sacred haunts, to waste in studious mood the midnight oil and grasp by efforts slow, severe, prolonged, some new truth

from the many labyrinth,—these will feel with Newton that their labors in the past, and all that any single man may hope to add, are to the contents of great Nature's book but as the gleanings of some little child gathering shells along an ocean strand.

Yet, notwithstanding this, the student rightly feels that every new truth is gathered for all time and is its own reward; and that, in spite of human error, prejudice and weakness, imperfect comprehension of the glories of the harvest and faulty applications of experience, progress is constant. Before the strong breath of persevering and repeated investigation, flake by flake, the chaff is winnowed away. That which remains, fit for the granary of knowledge, is imperishable. Let us then accept the lesson, in a social verse rounded by a poet naturalist, taught by the ever expanding camerated coil of pearly Nautilus, contemporary of the ages past as well as of the living present:—



VARIATIONS IN NATURE.

A CONTRIBUTION TO THE DOCTRINE OF EVOLUTION, AND THE THEORY OF NATURAL SELECTION. By THOMAS MEEHAN, of Germantown, Pa.

MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

You have honored me by a special invitation to address you on this occasion,[1] and you have suggested that I take for my theme some topic connected with plant life. I know that the papers I have given you during the past ten or fifteen years have led you to this kind consideration; and I therefore feel that I cannot do better than present to you now some of the impressions which my work has made on my own mind, in relation to some of the great questions of the day.

Perhaps the greatest of all questions is still the origin of species — the derivation of all the various forms we see — in brief, the doctrine of Evolution; and even where there is no doubt that one form has been evolved from some differing parent form, it has been by no means made so clear under what theory this growth of form is to be explained, that there can be no reasonable difference of opinion thereon. To my mind the doctrine of the origin of species as presented by Mr. Darwin cannot be controverted; and the theory of natural selection which he elaborated to sustain this doctrine requires only to be pruned of some abnormal growths to make it impregnable. This has been my task during the years referred to. Whenever I have seen weaknesses, I have adduced facts to oppose them. My task to-day is, as I hope, to show that the doctrine of evolution, and especially the theory of natural selection, is all the stronger for the work I have done.

[1] At the Cincinnati meeting the standing committee invited Mr. Meehan to occupy the chair of (the then) Section B, vacant through the inability of Dr. Geo. Engelmann, the elected Vice President, to be present. Mr. Meehan declined, fearing that his imperfect hearing would interfere with the proper performance of the duties of the chair. The Section then invited Mr. Meehan to deliver a special address at the next meeting of the Association.—ED.

I am particularly glad that the opportunity has been afforded me to undertake this pleasant task at this time, when the whole world is in mourning for Charles Darwin, whose chief claim to our love lies in the earnestness with which he pursued this very theme. I shall have to point out now as heretofore what I regard as weak points in his arguments, not to refute his main propositions, but in the belief that they will be stronger without them. I think you will regard it as one of the most fitting tributes to his memory if I succeed in showing that his propositions are stronger than he himself knew.

Darwin has shown that under domestication and under cultivation variations have occurred among animals and plants which we should certainly regard as species if we found them in a wild condition and their derivation were unknown. He shows that there are variations in nature, and by the operation of a law of selection the intermediate die, leaving forms which seem distinct to us because the intermediate have been lost. These forms continue to vary, with selections as before, till in time we have the great diversity we see. To this there is added another principle which he terms inheritance, and which counteracts variation in some degree, holding and retaining a given form to some extent, but never totally overawing the tendency to vary.

I know of nothing in my own observations, or in the observations of others to contradict this view. Facts confirming them are now of almost every-day occurrence, and we find them in the commonest plants. Near my home in southeastern Pennsylvania, we find the common Virginia creeper, *Ampelopsis quinquefolia*, with five leaflets. In Texas, Mr. Buckley finds it with seven, in northern Pennsylvania it is found sometimes with three. Along the Canada line it is mostly with five, but sometimes with seven. In southeastern Pennsylvania the leaflets are usually broadly ovate, slightly serrate, dark green, and the flowers and fruit are borne on rather stout pedicels; in Colorado the leaflets are rather wedge shaped, deeply laciniate, of a somewhat glaucous green, and the pedicels slender. In southeastern Pennsylvania the secondary veins are delicate, curved, and diverging from each other as they extend towards the edge of the leaflet; along the shores of Lakes Erie and Ontario, especially on Goat Island, near Niagara, the veins are very prominent, straight, almost parallel, and give the appearance at first sight of horse-chestnut leaves. We do not

regard these outlying forms as species, we do not even consider them as varieties. But why? Merely because we find in what I will call the central form, a tendency to all the characters referred to. If this central one were to disappear, I think botanists would have no difficulty in regarding the outlying forms as well marked varieties, if not good species. It may be said that these are properly the results of conditions and not variations; that they are the results of accidental circumstances, and are not perpetuated under hereditary laws; but it is one of the discoveries of modern times that these local variations, as they would once have been styled, are just as capable of being perpetuated as the most undoubted species. There is no difference whatever in this respect. Given a fair chance in the struggle for life for the seed to grow and the plant to mature seed, and the race at once has all it requires to go on and perpetuate itself.

It is just here that the advocacy of the theory of natural selection often shows a weakness which I distinguish from the theory itself. Mr. Darwin himself seemed to believe that variation in nature was a slow process, and he continually refers to the gradual modifications going on through long ages.[2] He contends that "variability is not an inherent and necessary contingency" of existence[3], "but comes from the law of correlation of growth; to the direct action of the conditions of life; to use and disuse; to the intercrossing of species;"[4] and he is especially emphatic in stating his belief that variations are ever in the line of profit to the individual. His leading idea is that those individuals which receive the greater sum-totals of profitable variations are the best able to maintain themselves in the struggle for life; in other words every act of life, variation included, proceeds on the basis of individual good. We have been made familiar with this view in much that has been said on the cross-fertilization of flowers. It has been said that flowers have been made beautiful solely on the basis of utility to the plants which bore them. It led to the visits of insects, which then led to cross-fertilization; and cross-fertilization is believed to be a means whereby the progeny is better able to battle for existence. Utility is with him the basis of all individual action; the impelling force in all change.

[2] Darwin's Origin of Species, Chap. IV, paragraph 5, X, paragraph 4, V, summary.
[3] Darwin's Origin of Species, Chap. V.
[4] Darwin's Origin of Species, Chap. V.

I feel sure, however, that a careful view of variations in nature will show that there is not the slightest physiological advantage in many a form which has had a wide distribution, over other forms which have but a localized existence. In the forms of *Ampelopsis* I have referred to, and which would become species if the central form were lost, any one form is just as able to do battle as another. Whether it succeeds in establishing itself or not depends wholly on other circumstances than matters connected with the character of its leaves or inflorescence, so far as we have been able to observe them. Or, let us take a race of plants of simple structure and of great variety. The family of cacti will serve for illustration. Supposing all these to have sprung from one primordial form, and granting that its spines are for protection, what possible gain has any one species obtained among the vast variety of form of plant or spines which enables it on that account to get along better than its neighbor? Supposing one species had a fascicle of twenty spines, how would it be the gainer by developing a new species with but ten, or another one with forty? The only argument I ever heard in such cases is that they must be individually beneficial or they would not exist. This is not the kind of reasoning that will satisfy us to-day, especially as a different view of the objects of nature will furnish us with a better answer.

What are the objects of nature? What will this world and all its creatures be like a hundred centuries from now? We see by the past as compared with the present that the one great aim is development. Nothing now is as it was then; and yet how closely connected is the present with the past! Then, as now, there was a struggle for life; but the gain to the conquerors was far less to them than to us. In one word it is posterity at which nature aims. It is not only those who die in the struggle who abandon life that others may live. It is only a few more years, as seen in the light of ages, before those for whom the others seemed to die follow after. Self-sacrifice and not self-interest is nature's demand on us all. We are here as Nature's invited guests; to do her work; to assist in this work of developing the future. She makes our stay as pleasant to us as possible; we should not do her work willingly unless she did, but she ruthlessly removes us the moment we are no longer of use to her in her plans of development. All nature is at work; but all this work would be vanity if it were merely for

individual good, and utility ended with individual life. Look at some poor mother toiling for her children, perhaps in poverty, and with but the barest necessaries of life to eat,—without rest or sleep that they may be fed and clothed; watching over them day by day in sickness and suffering, till her own health gives away, and she becomes a human wreck. She takes pleasure of course in this sacrifice; her lot would be truly unbearable if she did not. But wherein is the individual benefit? No! her struggle is not for life. It is self-sacrifice. She is aiding Nature in her great law of development. Her work is for posterity.

Just as you see it among human beings, you find it among all other animals. The most selfish of all will lay down its life for its young; its work like ours is for the future of its race, rather than for any good which may accrue to its own existence.

We find precisely the same laws operating in the vegetable as in the animal world. We discover nothing in the behavior of plants to indicate that they are actuated by individual good, further than may be necessary to enable them to fall in with Nature's great aim of preparing for the future. Millions of seeds are produced for every one that grows; millions grow for every one that lives long enough to flower; millions of flowers open for every one that yields seeds, and millions on millions of grains of pollen are produced for every one grain that is of service in fertilization. Under the individual utility theory all this is utter waste, and that plant life should be employed for, say, almost the whole of its existence, in sheer waste of effort solely that one moment might be possibly useful, is beyond all comprehension. It is customary to explain this by the guess that nature indulges in all this waste in order that there shall be no chance of failure. But even the thousandth part of all this waste would be sufficient for this permanent policy of insurance. Take for instance the sweet chestnut, *Castanea vesca*, count the number of stamens in a single flower of a male catkin, and multiply by the number of flowers the catkin bears; then get an idea of the number of male catkins on a single tree. Remember that each of the anthers contains hundreds of pollen grains, and conceive if you can the myriad millions of pollen grains on a single tree. Now look at your few bushels of chestnuts, and remember that a single pollen grain served for the production of each nut, and believe if you can that nature induced that tree to produce a million pollen grains in order that one

should surely do its work! Independently of this consideration we now know that these showy male flowers fall early, and, there is reason to believe, before the female flowers open, and that pollination is effected by a second crop of male flowers which appear simultaneously and on the same spike with the female. Moreover, we find, by what are known as cleistogene flowers, that nature can effect fertilization without any waste of pollen were she so minded. In *Viola* there is about a grain of pollen to each ovary in the cleistogamous flower. Another point has been suggested,—that this wonderful amount of vegetable exertion is to secure cross-fertilization, and this seems to be sustained by the small amount produced by cleistogene flowers; but allowing for all this it still does not account for the enormous waste. But we find these surplus seeds, surplus plants, surplus pollen, are useful, not to the parents which bore them, not in any way to themselves, but as sacrifices to posterity. They serve as food. They die that something else may live. They all work in with Nature's grand aim of developing something for the future. At the present time the eyes of science are turned to the past. We compare the dim view with that which is about us, and we perceive that all things have worked together for the good of the whole. We see that nothing has lived in vain. We know that in the general economy of nature there is no waste anywhere. Had the past turned to where we stand it would have seen nothing. People then would have had a better right to believe than we, that individual utility is at the bottom of all life; that "No. 1 is the first law of nature." It is for us to see that self-sacrifice was really their lot, and what was the future for them will still be the law of the future as the world rolls on.

To my mind, then, no doctrine of evolution, no theory of natural selection, will be perfect that looks to individual or immediate utility alone as its foundation-stone. Self-sacrifice, the good of the future at the expense of the individual or of the race, if need be, must hold the primal rank.

Let us now turn to look at nature. I am satisfied that not even the wonderfully acute powers of observation of the great Darwin led him to perceive the wonderful variations there are in vegetation everywhere. It is a common saying that things are "as much alike as two peas;" but if you look with a microscope you will not find two peas alike. No two leaves on a tree can be

found exactly alike. In outline, serration, pubescence, venation, some one thing or another you will find unlike the one you compare with.

Individual plants are not like other plants. You will be surprised if you compare closely to find how much they differ. It is best to take for examination those genera which have but a single species, so that no objections from cross-fertilization may arise. Take the ox-eye daisy, *Chrysanthemum Leucanthemum*, *Linaria vulgaris*, or *Epigœa repens*.[5] I name these because they are common, and because I have carefully examined them, and I have never found two plants from different seeds, but differed in some respect, either in foliage or inflorescence. You will find differences in cotyledons as well as in the primary leaves which succeed them. Examine a series of first leaves of *Robinia pseud-acacia* the common locust; some are elongated, some orbicular, some panduriform, some oblate, and these varying again in other characters. I have examined, for these facts, plants obtained from seed of the same tree. I also exhibit a series of *Pyrus*, or *Cydonia Japonica*, which I do know were raised from seed produced on one bush. There are some two hundred seedlings, and not only are no two alike, but there are amazing differences of form in the foliage, from narrow saliciform to almost round. Our esteemed fellow member and secretary of this meeting, Mr. William Saunders, showed me some seedling gooseberries of remarkable character. The seed was all from one berry, but the plants varied; some with very bristly fruit, some with smooth fruit, and I have myself seen ovate and globular fruited plants also raised from seed of one berry. I also exhibit two branches from two plants of *Rosa cinnamomea*, both plants raised from seed from one seed vessel, in which you see one branch almost wholly free of thorns and bristles; the other densely, nay ferociously, armed with long bristles and thorns. I could indefinitely continue these illustrations; you may at any time find numbers for yourselves. You will have no difficulty in believing that perpetual variation is a condition of existence. These variations have no special physiological value; no one will say that the possessor of one form has any special advantage over another in the struggle for life. Unless they are the incipient stages of that development which is to

[5] Proceedings Academy of Natural Sciences of Philadelphia, pp. 155-6.

result in species and genera in the future, they have absolutely no meaning at all.

Some who have noticed this fact of universal variation attach little importance to it. They believe it is but what they term a limited variation, extending only to a certain contracted line, and then returning; that such variations are at best but temporary and have no place under hereditary law. I confess that I once held this opinion. In a certain sense it is correct. If a leaf, oval, normally as we would say, changes until it becomes round, it cannot afterwards become rounder. A plant with a small flower develops one with a large flower. As flowers are morphologically changed leaves, a flower cannot become larger than the original leaf-type will permit. In this sense there must be a limit to variation, but in no other sense that I can find.

In the many combinations which can be made with a few elements, there is continuous change.

In regard to heredity I know from many years' experience, and contrary to generally received opinion, that any variation that appears in nature is capable of being perpetuated under the laws of heredity in just the same degree as those plants which we commonly regard as species. Let a white variety of a colored flower appear, and its progeny will continue white-flowered. I have had several generations of purple-leaved berberries and weeping peaches, and weeping ashes produce weeping seedlings. Purple peaches, golden spiræas, variegated plants of all kinds reproduce their kind with only just so much variation as we find in connection with all things.

I may remark further that sex, while to some extent an agent under the laws of variation, acts with greater force as an agent under heredity. Under the laws of variation A may depart somewhat from B on one side; and on the other side C departs from B; but an insect cross-fertilizing A with the pollen of C brings the progeny again towards B, and thus cross-fertilization acts as a check on rapid divergence. I have noticed this action of bees in the case of variations in *Linaria vulgaris*.* On the other hand there may be three variations, A, B, C; B being an advance on A, but B is fertilized by C; C having no opportunity to perpetuate itself dies out; but B succeeding in this has gained a step

*Proceedings of Academy of Natural Sciences of Philadelphia, 1868, p. 28.

through the sexual agency of C. To my mind the operation of the law of sex in connection with heredity, checking too great a tendency to wander off and mix with the world, producing as it were a sort of family relationship which it takes several generations under the law of variation to break up, is a far more legitimate theory of the use of cross-fertilization than anything which has been brought out under the more selfish hypothesis of utility to the individual or to its immediate descendants.

Now let us formulate our own theory of evolution and the origin of species by natural selection.

Nature primarily knows only the individual. This individual is made to reproduce itself after a short term of life, but with some general resemblance; yet every individual varies from its parent, some in one direction, some in another, the object of this variation being to reach some harmonious result far away in the future — a future which seems to us almost an eternity of years. The march of this variation is held in temporary check by heredity, aided especially by the influence of sex. Individuals become a large community. The wide world is around them. Those on the outside of the circle battle with the rest of the world; those in the interior battle also with themselves. For illustration we will call the community a pine forest. Millions of seeds are produced on the trees of those of the interior, not a hundred in a million will grow; of these hundred not ten may come to be young trees; and of these ten not more than one may get the chance to reproduce itself. Suppose that one happens to be what the human mind should regard as a striking variation; cross-fertilization steps in and draws it back to the original. No matter how great might be the physiological value of the variation to that tree in any ordinary struggle for life, antagonizing circumstances render the point gained of no possible use to it. But if the variation find itself on the outside of this circle, with no struggle with its own kind, with plenty of chance to mature its seed, an abundance of room for the young plants to develop, little opportunity for cross-fertilization to mar the distinctive variation it has gained, and still gain a chance for the progeny to reproduce in the same way, heredity will give you a distinct series, which will lead to arguments whether they are hybrids, varieties or species. If, however, acres of the interior of the forest get destroyed, and nothing but the outline of the circle is left, if there should arise a sort of Pacific Ocean over what was

once the land, and only, say Japan, remain on the west, and the Atlantic United States on the east, you would have only the modern variations left to tell the ancient story. It may not be the centre which meets this destructive fate. Some forms, as for instance *Onoclea sensibilis*, the well known fern, may avoid destruction, and get enwrapped in the laws of heredity for a hundred thousand years, while some or all of its progeny may have been swept away. There is no reason why grandparents among species may not survive long after every child or grandchild has ceased to be. The advance, the resting spot for the family, the further advance and the changes, the weeding out, and the survival of the fittest to carry on the great work of destiny,— how simple thus becomes the doctrine of evolution and the theory of natural selection!

We have taken the facts for our own conclusions as we have gone along; let us finally examine one more illustration. Here we have a series of cones made up from trees growing within a circuit of, say, a day's ride around Hammonton, N. J. They are all from trees of *Pinus rigida*. From one tree we have cones four inches long and almost round; others four inches long and not two inches wide; the one would be called almost linear, the other almost globose. Others are flat at the base and will stand upright; the others are rounded and roll over like marbles. One is so small as to be not more than an inch in length, and yet with perfect seeds. Some have very narrow scales, others have very wide ones. Some open slightly, but never much, others open so much that they reflex. Then there are some which to the best of my belief never open. Then we find several characters going along together. For instance, it is the narrow scaled forms which recurve; while the broader the scale the less tendency there is in the cone to open and scatter the seed. We see that all these cones belong to *Pinus rigida*, because between each cone there is but a shade of difference. Now I have already shown you that seeds of any one of these cones would produce trees bearing cones with a general resemblance to the parent cone. Supposing all these intermediates were to be crowded out, and only the two extremes left, there is not a botanist but would regard them as two distinct species. Now what I have given you as a suppositious case is an actual truth. We have at one end *Pinus rigida*, at the other *Pinus serotina* of the older botanists. *Pinus serotina* is no longer considered a good species. Why? Not because there was any error in the

original description, but simply because we have found the missing links. You cannot conceive of any physiological value to the individual or to the race to spring from any of these changes from the original form; you will not for one moment believe that in any one of these forms there is the slightest advantage to be gained over the other in what would be generally understood as the struggle for life. On the contrary, you will find one which has the cone closed, and in this particular lost the power to readily distribute the seeds; lost the great advantage of numbers in the tremendous battle; is the one form which has so well succeeded in establishing itself as to be the only one which has gained a distinctively botanical name.

I think you will come to agree with me that "the race is not always to the swift, nor the battle to the strong" is just as true in the development of species as in morals; that the whole round of organic existence is not one of self-seeking advantage over one's neighbor, but one of intrepid self-sacrifice; and that the doctrine of evolution and the theory of natural selection are rendered clearer and stronger by the perception of these facts.

I might rest here, but science cannot afford to ignore the objections sometimes made that evolution and especially the theory of natural selection is opposed to doctrines which are held sacred by so large a portion of the human race. I would rather that we could ignore them. Science should be allowed to work out its own salvation, if there be aught in it worth saving. It would die of itself when it had nothing to live on; but objections being made it is a gain to science to show that they have no force.

The essential doctrine of Christianity is self-sacrifice. It was the great burden of its founder's teaching, and was illustrated by the sacrifice of his own life. True happiness to him was to do the will of Him who sent him.

How exactly does this not accord with the laws of nature as I have unfolded them? Variation, as we have seen, is established, not that things which vary derive any special advantage by the variation, but that all varied things may work the better together for the good of some future object, to them unknown. Their work is of no advantage to them, but the task is made pleasant, so that the work may still go on.

And the reaper, death, gathers in his harvest of species, just as he gathers in his harvest of individuals, without regard to youth or

old age, to the strong or to the weak, oftentimes confounding our reason by the mere assertion of his will, and leaving us no alternative but to believe that natural selection, true as it is and must be, is still in the hands of some directing power which we have not yet fathomed, but which is making all things work together for some great future object, of which, as yet, not the faintest glimmer has been perceived by the wisest man.

There is no conflict between the doctrine of evolution with its theory of natural selection and the essential doctrines of Christianity. Rather, as I believe, will the day arrive when those who advocate these views will be regarded as Christianity's best friends.

PAPERS READ.

REMARKS CONCERNING THE FLORA OF NORTH AMERICA. By ASA GRAY, of Cambridge, Mass.

IN the remarks which I have to offer to this Section, you will understand the word *Flora* to be written with a capital initial. I am to speak of the attempts made in my own day, and still making, to provide our botanists with a compendious systematic account of the phænogamous vegetation of the whole country which the American Association calls its own.

I shall make no effort to avoid the personal turn which my narrative is likely to take. In fact, it will be seen that I have partly a personal object in drawing up this statement.

Only two Floras of North America have ever been published as completed works, that of Michaux and that of Pursh. A third was begun (by Dr. Torrey, assisted by a young man who is no longer young), by the publication in the summer of 1838 of a first fasciculus; the first volume of 700 pages was issued two years afterward; and 500 pages of the second volume appeared in 1841 and in the early part of 1843. The time for continuing it in the original form has long ago passed by. Its completion in the form in which I have undertaken it anew, is precarious. *Precarious* in the original sense of the word, for it is certainly to be prayed for: precarious, too, in the current sense of the word as being uncertain; yet not so, according to an accepted definition, viz.: "uncertain, because depending upon the will of another;" for it is not our will but our power that is in question; and it is only by the combined powers and efforts of all of us interested in Botany that the desired end can possibly be attained.

It were well to consider for a moment how and why it is that a task which has twice been — it would seem — easily accomplished has now become so difficult.

The earliest North American Flora, that of the elder Michaux, appeared in the year 1803. It was based entirely upon Michaux's own collections and observations, does not contain any plants which he had not himself gathered or seen, is not, therefore, an

exhaustive summary of the botany of the country as then known, and so was the more readily prepared. Michaux came to this country in 1785, returned to France in 1796, left it again in Baudin's expedition to Australia in 1800, and died of fever in Madagascar in 1802. The Flora purports to be edited by his son, F. A. Michaux, who signed the classical Latin preface. The finish of the specific characters, and especially the excellent detailed characters of the new genera, reveal the hand of a master; and tradition has it that these were drawn up by Louis Claude Richard, who was probably the ablest botanist of his time. This tradition is confirmed by the fact that Richard's herbarium (bequeathed to his son, and now belonging to Count Franqueville), contains an almost complete set of the plants described, and I found that the specimens of Michaux supplied to Willdenow's herbarium at Berlin were ticketed and sent by Richard. Not only the younger Richard but Kunth also habitually cited the new genera of the work as of Richard, and some others have followed this example. Singularly enough, however, there is no reference whatever to Richard in any part of the Flora, nor in the elaborate preface. The most venerable botanist now living told me that there was a tradition at Paris that Richard performed a similar work for Persoon's *Synopsis Plantarum*, and that he declined all mention of his name in the synopsis and in the Flora, because the two works — contrary to the French school — were arranged upon the Linnæan Artificial System. He had his way, and the tradition may be preserved in history; but his name cannot be cited for the genera *Elytraria*, *Micranthemum*, *Elodea*, *Stipulicida*, *Dichromena*, *Oryzopsis*, *Erianthus*, and the like. For, by the record these are of *Michaux*, *Flora Boreali-Americana*, and not of Richard.

Michaux's explorations extended from Hudson's Bay, which he reached by way of the Saguenay, to Florida, as far, at least, as St. Augustine and Pensacola; he was the first botanical explorer of the higher Alleghany Mountains, and, crossing these mountains in Tennessee, he reached the Mississippi in Illinois, and was as far south as Natchez. His original itinerary, which I once consulted, is preserved by the American Philosophical Society, at Philadelphia, to which it was presented by his son. It ought to be printed. That little journal shows that it was not Michaux's fault that the first Flora of North America was restricted to the district east of the Mississippi River. He had a scheme for

crossing the continent to the Pacific. He warmly solicited the government at Washington to undertake such an exploration, and offered to accompany it as naturalist. This may have been the germ or the fertilizing idea of the expedition of Lewis and Clark, which was sent out a few years afterward by Jefferson, to whom, if I rightly remember, Michaux addressed his enterprising proposal.

Leaving out the Cryptogams of lower rank than the Ferns, we find that the Flora of Michaux, published at the beginning of this century, say eighty years ago, contains 1530 species, in 528 genera. No very formidable number; as to species (speaking without a count) little over half as many as are described in my Manual of the Botany of the Northern States, which covers less than half of Michaux's area.

Eleven years afterward, namely, in the year 1814 (the preface is dated December, 1813), appeared the second Flora of North America, namely the *Flora Americæ Septentrionalis* by Frederick Pursh. This was not confined to the author's own collections, but aimed at completeness, or to give "a systematic arrangement and description of the plants of North America, containing, besides what have been described by preceding authors, many new and rare species, collected during twelve years' travels and residence in that country."

It appears that Pursh was born at Tobolsk, in Siberia, of what parentage we do not know. He himself tells us, in his preface, that he was educated in Dresden, and he came to this country — to Baltimore and Philadelphia—at the close of the last century, when he must have been only twenty-five years old. He was able to make the acquaintance not only of Muhlenberg, who survived until 1815, and of Wm. Bartram, who died in 1823, but also of the veteran Humphrey Marshall, who died in 1805. His early and principal patron was Dr. Benjamin Smith Barton, who supplied the means for most of the travels which he was able to undertake, and who, as Pursh states, "for some time previous had been collecting materials for an American Flora." Pursh's personal explorations were not extensive. From 1802 till 1805 he was in charge of the gardens of Wm. Hamilton, near Philadelphia. In the spring of the latter year, as he says, he "set out for the mountains and western territories of the Southern States, beginning at Maryland and extending to the Carolinas (in which tract the

interesting high mountains of Virginia and Carolina took my particular attention), returning late in the autumn through the lower countries along the sea-coast to Philadelphia." But, in tracing his steps by his collections[1] and by other indications, it appears that he did not reach the western borders of Virginia nor cross its southern boundary into the mountains of North Carolina. The Peaks of Otter and Salt-pond Mountain (now Mountain Lake) were the highest elevations which he attained. Pursh's preface continues: "The following season, 1806, I went in like manner over the Northern States, beginning with the mountains of Pennsylvania and extending to those of New Hampshire (in which tract I traversed the extensive and highly interesting country of the Lesser and Great Lakes), and returning as before by the sea-coast." The diary of this expedition, found among Dr. Barton's papers and collections in possession of the American Philosophical Society, has recently been printed by the late Mr. Thomas Potts James. It shows that the journey was not as extended or as thorough as would be supposed: that it was from Philadelphia directly north to the Pokono Mountains, thence to Onandaga, and to Oswego,—the only point on the Great Lakes reached,—thence back to Utica, down the Mohawk Valley to Saratoga, and north to the upper part of Lake Champlain and to the lesser Green Mountains in the vicinity of Rutland, but not beyond. Discouraged by the lateness of the season, and disheartened—as he had all along been—by the failure and insufficiency of remittances from his patron, Pursh turned back from Rutland on the 22d of September, reached New York on the 1st of October, and Philadelphia on the 5th. The next year (1807) Pursh took charge of the Botanic Garden which Dr. Hosack had formed at New York and afterward sold to the State, which soon made it over to Columbia College.[2] In 1810, he made a voyage to the West Indies for the recovery of his health. Returning in the autumn of 1811, he landed at Wiscasset, in Maine, "had an opportunity of visiting Professor Peck of Cambridge College, near Boston," and of seeing the alpine

[1] In both Barton and both Lambert.

[2] Expecting, no doubt, that it would be kept up. But "the Elgin Botanic Garden" was soon discontinued. It occupies the block of ground now covered by the buildings of the College, and the surrounding tract—now so valuable—from which the College derives an ample revenue. Noblesse oblige, and it may be expected that the College—so enriched—will, before long, provide itself with a botanical professorship, and see to the careful preservation and maintenance of the precious Torrey Herbarium, which it possesses along with other subsidiary herbaria.

plants which Peck had collected on the White Mountains.[3] At the end of the latter year or early in 1812 he went to England with his collections and notes; and at the close of 1813, under the auspices of Lambert, he produced his Flora, consulting, the while, the herbaria of Clayton, Pallas, Plukenet, Catesby, Morison, Sherard, Walter, and that of Banks. Evidently such consultations and the whole study must have been rapid. The despatch is wonderful. One can hardly understand the ground of the statement made by Lambert to my former colleague, Dr. Torrey, that he was obliged to shut Pursh up in his house in order to keep him at his work.

I know not how Pursh was occupied for the next four years, nor when he came to Canada. But he died here at Montreal, in 1820, at the early age of forty-six. More is probably known of him here. If I rightly remember, his grave has been identified, and a stone placed upon it inscribed to his memory.[4] A tradition has come down to us — and it is partly confirmed by a statement which Lambert used to make, in reference to the vast quantity of beer he had to furnish during the preparation of the Flora—that, in his latter days, our predecessor was given to drink, and that his days were thereby shortened.

In Pursh's Flora we begin to have plants from the Great Plains, the Rocky Mountains, and the Pacific Coast, although the collections were very scanty. The most important one which fell into Pursh's hands was that of about 150 specimens, gathered by Lewis and Clark on their homeward journey from the mouth of Columbia River. A larger collection, more leisurely made on the outward

[3] It is at Wiscasset, therefore, that Pursh's "*Plantago cucullata* Lam. . . . . in wet rocky situations, Canada and Province of Maine," is to be sought. Mr. Pringle has recently found the related *P. Cornuti* (which may be the plant meant), in Lower Canada, not far from the other side of Maine.

It must have been in Professor Peck's herbarium (no longer extant), that Pursh saw what he took to be *Alchemilla alpina*, which he remarks "*v. s.*" and refers to from memory only, probably mistakenly. For it has not since been detected either in Vermont or New Hampshire, or anywhere in North America; and Pursh's Journal makes it certain that he did not reach any alpine region in the Green Mountains.

[4] In the Canadian Naturalist, Principal Dawson gives a brief account of the transference of the remains of Pursh from a grave-yard below Montreal, in which they were interred, to the beautiful Mount Royal Cemetery, where they rest in a lot purchased for the purpose and under a neat and durable granite monument, provided by the naturalists of Montreal and their friends. A small company of botanists, led by Dr. Dawson, visited the spot shortly after the reading of this paper. We learned that Pursh had botanized largely in Canada, in view of a Canadian Flora, and that his collections were consumed by a fire at Quebec shortly before his death, to his extreme discouragement.

journey, was lost. Menzies in Vancouver's voyage had botanized on the Pacific coast, both in California and much farther north. Some of his plants were seen by Pursh in the Banksian Herbarium, and taken up. I may here say that in the winter of 1838–39 I had the pleasure of making the acquaintance of the venerable Menzies, then about ninety-five years old.

In the Supplement, Pursh was able to include a considerable number of species, collected by Bradbury on the Upper Missouri, in what was then called Upper Louisiana,—much to the discontent of Nuttall, who was in that region at the same time, and who, indeed, partly and imperfectly anticipated Pursh in certain cases, through the publication by the Frasers of a catalogue of some of the plants collected by Nuttall.

To come now to the extent of Pursh's Flora, published nearly sixty-nine years ago. It contains 740 genera of Phænogamous and Filicoid plants, and 3076 species,—just about double the number of species contained in Michaux's Flora of eleven years before.

I must omit all mention of more restricted works, even such as Nuttall's Genera of North American Plants, which came only four years after Pursh's Flora: also the Flora Boreali-Americana of Sir Wm. Hooker, which began in 1829, but was restricted to British America. I cannot say how early it was that my revered master, Dr. Torrey, conceived the idea of the Flora which he at length undertook. But he once told me that he had invited Nuttall to join him in the production of such a work, and that Nuttall declined. This must have been as early as the year 1832, that is, half a century ago. My correspondence with Dr. Torrey began in the summer of 1830, when I was a young medical student, and three or four years afterward I joined him at New York and became, for a short time, his assistant, for all the rest of his life his botanical colleague. He was very much occupied with his duties as professor, chiefly of chemistry; he had not yet abandoned the idea of completing his Flora of the Northern and Middle States, the first volume of which was finished in 1824, while yet free from all professional cares. Although working in the direction of the larger undertaking, the *Flora of North America* did not assume definite shape before the year 1835. I believe that some of the first actually-prepared manuscript for it was written by myself in that or the following year. I was then and

for a long time expecting to accompany the South Pacific Exploring Expedition, as originally organized under the command of Commodore Ap. Catesby Jones, but which was subject to long delay and many vicissitudes; during which, having plentiful leisure, I tried my 'prentice hand upon some of the earlier natural orders. Before the expedition, as modified, was ready to sail, under the command of Capt. Wilkes, I had accepted Dr. Torrey's proposal that I should be his associate in the work upon which I had made a small beginning as a volunteer. Two parts, or half of the first volume (360 pages), of this Flora, were printed and issued in July and October, 1838.

It was thought at first, in all simplicity, that the whole task could be done at something like this rate. But, apart from other considerations, it soon became clear that there had been no proper identification of the foundation-species of the earlier botanists, from Linnæus downward; and that our Flora could not go on satisfactorily without this. Dr. Torrey had, indeed, some years before, made a hasty visit to Hooker at Glasgow, to London, and to Paris; but the taking of a few notes upon some particular plants in the herbaria of Hooker, Lambert, and Michaux, and the acquisition, from Hooker, of a good set of the Arctic plants of the British explorers, was about all that had been done. I proposed to attempt something more; so, taking advantage of a favorable opportunity, I sailed for Liverpool in November, 1838, and devoted a good part of the ensuing year to the examination of the principal herbaria, which I need not here specify, in Scotland (where the important one of Sir Wm. Hooker still remained), England, France, Switzerland and Germany, namely, those which contained the specimens upon which most of the then-published North American species had been directly or indirectly founded, especially those of Linnæus and Gronovius, of Walter, of Aiton's Hortus Kewensis, Michaux, Willdenow, Pursh, and the later ones of DeCandolle and Hooker.[5]

After my return the work made good progress; the remaining half of the first volume was brought out in the spring of the year 1840, and by the spring of 1843 the 500 pages of the second volume, mostly occupied by the vast order Compositæ, had been issued. But meanwhile I had in my turn to assume professorial

[5] See, in this connection, "Notices of European Herbaria particularly those most interesting to the North American Botanist," in this Journal, vol. xi, January, 1840.

duties and incident engagements,—with the result that, although the study of North American plants was at no time pretermitted, either by Dr. Torrey while he lived, or by myself, we were unable to continue the publication during my associate's lifetime; and it was only recently, in the spring of 1878, that I succeeded in bringing out, in a changed form, another instalment of the work, completing the *Gamopetalæ*.

In the interval I had made two year-long visits to Europe for botanical investigation, the first partly relating to the botany of the South Pacific, the second wholly in view of the North American flora. And since this last publication still another visit—the fourth and we may suppose the last—of the same character and the same duration, has been successfully accomplished.

The serious question, in which we are all concerned, arises, whether this work can be carried through to a completion, and the older parts (wholly out of print and out of date), reëlaborated —I will not say by my hands—but in my time, or soon enough to render the whole a reasonably full and homogeneous representation of the North American flora, as known in this latter part of the nineteenth century. And it brings us to consider why the undertaking, to which so much time has been devoted, should be so slow of accomplishment.

If this slowness is a constant wonder and disappointment to most people interested in the matter, I can only add that it is hardly less so to myself. It is a constant surprise—if one may so say—that the work does not get on faster.

Of course the undertaking has become more and more formidable with the enlargement of geographical boundaries and of the number of species discovered. As to the increase in the number of species to be treated, we have by no means yet reached the end. The area, that of our continent down to the Mexican line, we trust is definitely fixed, at least for our day. And, since we cannot be rid of the peninsula and keys of Florida, which entail upon us a considerable number of tropical species, mostly belonging to the West Indies—the southern boundary is now as natural a one as we can have.

The area which Pursh's Flora covered was, we may say, the United States east of the Mississippi, with Canada to Labrador, to which was added a couple of hundred of species known to him outside these limits northwestward.

Torrey and Gray's Flora took the initiative in annexing Texas, ten years before its political incorporation into the Union; although the only plants we then possessed from it were certain portions of Drummond's collections. California was also annexed at the same time, on account of Douglas's collections, and those of Nuttall, who had just returned from his visit to the western coast, which he reached by a tedious journey across the continent over ground in good part new to the botanist. Douglas had already made remarkably full collections along a more northern line. The British arctic explorers, both by sea and land, had well developed the botany of the boreal regions, and Sir Wm. Hooker was bringing out the result in his Flora of British America. Of course our knowledge of the whole interior and western region was small indeed, compared with the present; and the botany of a vast region from the western part of Texas to the Californian coast was absolutely unknown, and so remained until after the publication of the Flora was suspended.

As to the number of species which Torrey and Gray had to deal with, I can only say that a rapid count gives us for the first volume about 2200 Polypetalæ; that there are 109 species in the small orders which in the second volume precede the *Compositæ;* and that there are of the *Compositæ* 1054. So one may fairly conclude that if the work had been pushed on to completion, say in the year 1850, the 3076 species of Pursh's Flora in the year 1814 might have been just about doubled. Probably more rather than less; for if we reckon from the number of the *Compositæ*, and on the estimate that they constitute one-eighth of the phænogamous plants of North America, instead of 6150, there would have been 8430 species known in the year specified.

It most concerns us to know the number of species which, after the lapse of thirty years more—years in which exploration has been active, and has left no considerable part of our great area wholly unvisited—the now revived Flora has to deal with. We can make an estimate which cannot be far wrong. In the year 1878, my colleague, Mr. Watson, finished and published his Bibliographical Index to the Polypetalæ of North America, covering, that is, the same ground as the first volume of Torrey and Gray's Flora, completed in 1840. In it the 2200 species of the latter date are increased to 3038. The "*Gamopetalæ* after *Compositæ*" in the Synoptical Flora, brought out in the same year,

contains 1656 species. The two together must make up half of our phænogamous botany, that is, adding the increase of the last four years, about 5000 species. And so Mr. Watson adopts the estimate of 10,000 species for our known Phænogams and Ferns. My expression is that the species of *Compositæ* have increased at a rate which, unless they exceed the eighth part of our Phænogams, will warrant a still higher estimate. The number of introduced species of various orders, which will have to be enumerated and most of them described, is, unhappily, fast increasing,[6] and new indigenous species are almost daily coming to us from some part or other of our wide territory; so that the 10,000 species of this estimate may before long rise to eleven or twelve thousand. Only the experienced botanist can form a just idea of what is involved in the accurate discrimination and proper coördination of 10–12000 species, and in the putting of the results into the language and form which may make our knowledge available to learners or to succeeding botanists.

Moreover, there is of late an *embarras des richesses* which is becoming serious as respects labor and time. The continued and ever increasing influx of materials to Cambridge, beneficial as it ever is, is accountable for this retardation of progress in a greater degree than almost any one would suppose. The herbarium, upon whose materials this work is mainly done, and which has been, like the Temple, full forty and six years in building, has received the contributions of two generations of botanists, and the Torrey herbarium goes back one generation further. Still the number of American specimens annually coming to it is greater than in most former years. Apart from the mere selection and care of these, consider how in other ways it affects the rate of progress of the Flora. The incoming of additional specimens may at a glance settle doubts as to the validity of a species; but new specimens are as apt to raise questions as to settle them; more commonly they raise the question as to the limitation and right definition of the species concerned, not rarely, also, that of their validity. When one has only single specimens of related species, the case may seem clear and the definition easy. The acquisition of a few more, from a different region or other conditions, almost always

[6] I say "unhappily," for they adulterate the natural character of our flora, and raise difficult questions as to how much of introduction and settlement should give to these denizens the rights of adopted citizens.

calls for some re-consideration, not rare for re-construction. People generally suppose that species, and even genera, are like coin from the mint, or bank notes from the printing press, each with its fixed marks and signature, which he that runs may read, or the practised eye infallibly determine. But in fact species are judgments—judgments of variable value, and often very fallible judgments, as we botanists well know. And genera are more obvious judgments, and more and more liable to be affected by new discoveries. Judgments formed to-day—perhaps with full confidence, perhaps with misgiving—may to-morrow, with the discovery of new materials or the detection of some before unobserved point of structure, have to be weighed and decided anew. You see how all this bears upon the question of time and labor in the preparation of the Flora of a great country. If even in old Europe the work has to be done over and over, how much more so in America, where new plants are almost daily coming to hand. It is true that these fall into their ranks, or are adjustable into their proper or probable places, but not without pains-taking and tedious examination.

Of our Flora, it may indeed be said, that "if 'twere done when 'twere done, then 'twere well it were done quickly." But I may have made it clear that, in the actual state of the case, it is likely to be done slowly. At least you will understand why thus far it has been done slowly. As to the future, if it depended wholly upon me, the completion would obviously be hopeless. I need not say that our dependence for the actual elaboration must largely be upon associates, upon the few who have the training and the vast patience, and the access to herbaria and libraries, requisite for this kind of work, but above all upon my associate in the herbarium at Cambridge, to whom, being present with us, I will not further allude.

Of course we rely, very much indeed, upon the continued coöperation of all the cultivators of botany in the country; and it is gratifying to know that their number is increasing, new ones not less zealous than the old, and better equipped, are taking the places of those that have passed away, and some of them extending their explorations over the remotest parts of the land, and into districts where there is most to be discovered. All can help on the work, and all are doing so, by the communication of specimens and of observations. Those within the range of the pub-

lished manuals and floras get on—or should get on—with only occasional help from us. They should send us notes and specimens to any amount; but they should not ask us to stop to examine and name their plants, except in special cases, which we are always ready enough to take up. Those who collect in regions as yet destitute of such advantages may claim more aid, and we take great pains to render it; partly on our own account, that we may assort their contributions into their proper places, partly for the encouragement of such correspondents, who otherwise would not know what they have obtained, and who naturally like to know when they have made interesting discoveries.

But the scattered and piecemeal study of plants is neither very satisfactory nor safe. And it involves great loss of time, besides interrupting that continuity and concentration of attention which the proper study of any group of plants demands. As respects the orders of plants which are yet to be elaborated for the Flora, and as to plants which require critical study or minute examinations, necessarily consuming much time, it is better to defer their complete determination until the groups to which they severally belong are regularly taken in hand.

The coöperation of all our botanical associates is solicited in this regard, as a matter of common interest and advantage. For we are all equally concerned in forwarding the progress of the Flora of North America; and we may confidently expect from our botanical associates their sympathy, their forbearance, and their continued aid.

On the Position of the Gamopetalæ. By Lester F. Ward, of Washington, D. C.

[ABSTRACT.]

In 1789, Laurent de Jussieu, in his "Genera Plantarum," established the three "Divisions" of Dicotyledons, assigning the Apetalous Division the lowest place, the Monopetalous Division the second, or intermediate, place, and the Polypetalous Division the highest place in the series.

In 1848, Adrien de Jussieu, in his "Cours Eléméntaire de

Botanique," reversed the order of the second and third of these Divisions and gave his reasons therefor.

Notwithstanding this, nearly all subsequent authors have steadily adhered to the order established by Laurent de Jussieu, which is supposed to have been that of his uncle Bernard, the real founder of the "natural system."

This arrangement has, however, been severely criticised by Al. Braun and many others.

The present paper is designed to supplement an article "On the Natural Succession of the Dicotyledons" communicated by the writer to the "American Naturalist" for November, 1878, and to bring forward some evidence from statistics and from palæontology, as that did from organogeny, of the comparative *recency* of those plants having tubular, bell-shaped, or otherwise consolidated corollas, and thus to claim for that Division of the Dicotyledons — the *Monopetalæ*, or *Gamopetalæ*, as they are now more properly called — the highest place in the series, as did the younger Jussieu.

This is shown, first, by the great extent to which these plants are herbaceous as compared with those having the petals free (*Polypetalæ*) or with those destitute of petals (*Apetalæ*, or *Monochlamydeæ*).

From a comparison of seventeen floras, including those of the Eastern United States, of California, of Great Britain, of Italy, of the West Indies, etc., it is found that of the total Dicotyledons the Apetalous Division furnishes on an average 13.3 per cent, the Polypetalous Division 43.3 per cent, and the Gamopetalous Division 43.4 per cent; but when the shrubs, trees, and woody vines of each of these Divisions are alone considered, we find that the *Apetalæ* furnish about 24 per cent, the *Polypetalæ* over 43 per cent, and the *Gamopetalæ* only about 33 per cent. In some of the floras compared the percentage of this last Division is as low as 20.

A perhaps still more striking way of showing this is to see what proportion the *woody plants* of each Division bear to the total number in that Division; *e. g.*, of the *Apetalæ*, which in the flora of the Eastern United States form only 13 per cent of the total Dicotyledons, 42 per cent are woody, while of the *Gamopetalæ*, which form 47 per cent of the Dicotyledons, only 16 per cent are woody.

If trees only are taken into the account the contrast becomes

still stronger. In the last named flora, which is the largest of those compared, the *Apetalæ* constitute 38, the *Polypetalæ* 45, and the *Gamopetalæ* 17 per cent of the dicotyledonous trees.

These facts seem to show that the *Gamopetalæ* constitute a comparatively late type of vegetable growth which has not yet had time, as it were, to perfect itself and assume the frutescent and arborescent forms which mark the highest development, or phytological maturity.

In the second place, and in order to anticipate the possible objection that this might equally prove degeneracy, such as is seen in the Monocotyledons and in the Cryptogams, which, though known to be older types of vegetation, are, nevertheless, at the present time chiefly herbaceous, although once probably for the most part arborescent, the following palæontological evidence was adduced:

It was shown

1. That the first appearance of plants that are clearly referable to Gamopetalous genera is much later than that of either Apetalous or Polypetalous plants.

2. That the most abundant of those genera are such as in the living flora have very deeply lobed corollas, as in *Viburnum*, thus approaching the dialypetalous condition and strongly suggesting that at the time of their appearance they might not have been truly gamopetalous at all.

3. That the relative paucity of gamopetalous species is much more marked in fossil than in living floras.

4. That the lower the horizon, the greater the disproportion in this respect.

5. That in all collections the number of *specimens* of fossil is less than of living *Gamopetalæ* in proportion to the number of species; *i. e.*, there were then not only less numerous forms, but those that existed were less abundant.

These several propositions were supported by copious citations and by tubular exhibits compiled from Schimper's "Paléontologie Végétale," Heer's "Flora tertiaria Helvetiæ," Lesquereux's Cretaceous and Tertiary Floras of the United States, and other works, as well as by data obtained from the collections of fossil plants at the United States National Museum.

A REVISION OF THE GENUS CLEMATIS OF THE UNITED STATES. By JOSEPH F. JAMES, of Cincinnati, Ohio.

[ABSTRACT.[1]]

THE paper contains descriptions of all the species of Clematis native to the United States. Following the descriptions are remarks relative to them and the following changes are suggested in the nomenclature. *Clematis ovata* Pursh is reduced to a synonyme of *C. ochroleuca* Aiton; *C. Fremontii* Wats, is reduced to a variety of *C. ochroleuca* Ait.; *C. coccinea* Engelm. is referred back as a variety of *C. Viorna* L.; *C. Pitcheri* Torr. and Gr. is considered a variety of *C. Viorna* L.; *C. Catesbyana* Pursh and *C. pennsylvanica* Donn are considered synonymes of *C. virginiana* L.; *C. holosericea* Pursh is considered var. *bracteata* D. C. of *C. virginiana* L. Following the descriptions is given the geographical distribution of each of the species, and after this remarks on the probable place of origin of some of the species. The paper concludes with a synonymy of the species.

ACHENIAL HAIRS AND FIBRES OF COMPOSITÆ. By G. MACLOSKIE, of Princeton, N. J.

[ABSTRACT.[2]]

THE surface of the achenes, the character of their hairs, and their internal structure, are used to indicate the affinities of the suborders, according to the following tabular scheme.

I. Vernonieæ; as in Asteroideæ.

II. Eupatorieæ; "

III. Asteroideæ; achenes bearing duplex hairs, which are usually bifurcate at tips, acute, and often unequal. They have no elaters. Hairs sometimes few or obsolete; but even in such cases rudiments of duplex hairs may occur. Some are as in Inuloideæ.

IV. Inuloideæ; duplex hairs, usually obtuse and equal. No elaters.

V. Helenioideæ; as in Asteroideæ. Crystalloids occur in the endocarp.

VI. Anthemideæ; achenes usually glabrous: but having per-

[1] Printed in full in Jour. Cincinnati Soc. Nat. Hist., Vol. vi, July, 1883.

[2] Published in full, with illustrations, in the AMERICAN NATURALIST for Jan., 1883.

icarp-cells with spiral filaments (Achillæa has internal glands in pericarp-cells.)

VII. Senecionidæ; duplex hairs, having divisions equal, with elaters on filaments which escape when moistened.

VIII. Calenduleæ; probably as in Senecionidæ (in some cases with multicellular hairs interposed).

IX. Arctotideæ; as in Asteroideæ.

X. Cynaroideæ; some (*Carlina*, *Xeranthemum*) as in Asteroideæ; some (*Cnicus*, etc.) as in *Cichorieæ*. Some have simple hairs of perianth encroaching on achene (*Arctium*, *Centaurea*, *Echinops*).

XII. Mutisieæ; as in Asteroideæ.

XIII. Cichorieæ; achenes glabrous, with denticulate epidermal cells. Endocarp having filaments enclosing crystalloids. These by pressing the seed-vessel of dandelion cause it to dehisce and protrude the seed.

These observations suggest a somewhat different line of affinities from that usually adopted. The Asteroid group should include most of the suborders. Anthemideæ are misplaced: and the tribal unity of Cynaroideæ is questionable, one section of its genera agreeing as to their achenes with Cichorieæ.

Some Observations on the Action of Frost upon Leaf-cells.

By C. E. Bessey, of Ames, Iowa.

[ABSTRACT.]

The severe frosts of May 22 and 23, 1882, in Central Iowa, gave me the opportunity of making some important observations on the action of frost upon the cells of the young leaves of certain trees. The leaves of the Green Ash (*Fraxinus viridis*) were just developing, and were in that tender state peculiar to such young structures. Many of the leaflets were entirely killed, while perhaps the great majority were simply blackened, that is, they were killed near the apex, while toward the base they were still alive.

The examination of thin sections of these browned leaves soon after the frost, say within forty-eight or sixty hours, showed that the cells were differently affected by the low temperature. Instead of all the cells being equally injured, some were found to be dead,

and in these the protoplasm was brown and shrunken; in others the contents showed more or less of injury by a greater or less coagulation of the still green protoplasm; others, again, appeared to be scarcely injured.

The disappearance of the chlorophyll granules, as distinct bodies, was one of the most common phenomena in the less injured cells. In many cells, which apparently were still alive, the colored protoplasm appeared to have retracted itself from the outer cell walls, toward the inner part of the cell, that is, the part of the cell lying toward the interior of the leaf. This was more particularly noticeable in the palisade cells of the leaf section.

Some of these leaves were affected by the Ash-leaf Rust, the so-called *Æcidium fraxini* of Schweinitz, forming small orange-yellow spots in the parenchyma or the fibro-vascular bundles of the ribs. The leaf-tissues are always thickened in these spots so that the thickness of the leaf in them is double or treble that in the normal portions. Under the microscope the tissues of the spots are seen to be penetrated by ramifying hyphæ of the parasite, which also extend for some distance into the surrounding, still green, tissues.

After the frost, I noticed within a few hours, that the cells immediately surrounding the orange spots were somewhat translucent, giving early evidence of their having been more injured than the adjacent outer tissues. Upon waiting a day or two, I observed that these translucent cells turned black and began to decay. Sections made both in the earlier and later stages showed that in this surrounding belt of cells the protoplasm had been much more severely injured than in those lying farther away from the spots.

The observations here recorded appear to indicate that in many plant cells there is the power of resisting (however slightly) the injurious effects of low temperature, and that in this respect there is a difference between cells which occupy, so far as we may see, similar positions, and which are equally exposed to the action of the frost. Moreover, it appears to be demonstrated that the lowering of the vitality of cells, as by the presence of parasitic hyphæ, lowers, also, their power of resisting low temperature.

The importance of these observations consists in furnishing *experimental* proof of the power of living plant cells to resist destruction by low temperature; hitherto the doctrine has been held on theoretical grounds mainly.

The motions of Roots and Radicles of Indian Corn and of Beans. By W. J. Beal, of Lansing, Mich.

C. Darwin showed that a piece of gummed paper on one side of the tip of a root in damp air caused it to bend out of its course. At the Cincinnati meeting I showed that roots in damp air often made coils without anything being placed on the tips. In germinating some 700 kernels of yellow dent corn in the soil, not one root made a coil. The roots seldom made very abrupt turns in any direction.

In a cool, damp cellar in August, kernels of Indian corn were planted with one end uppermost and the surface of sand pressed down smooth. They were placed in sand about three-fourths of an inch below the surface. The plumules all turned up and came out of the soil. Primary and secondary roots came out of the sand and often run along on the surface for a foot or more. The roots often entered the sand again after coming out. When planted with the *apex* of the kernel uppermost, secondary roots came out most numerous; often two or three to each plant. When planted with the radicle end uppermost the primary root often came out of the ground. Of 150 kernels with the radicle end down three sent the primary root out of the sand.

Of 181 kernels where the radicle was planted uppermost, eighty-nine sent primary roots out of the sand. Whether out of the sand or in it, the primary roots for a foot or more always kept ahead of the secondary roots. The roots apparently grew just as well out of the sand as when beneath the surface. Those above the soil soon became red. When planted in sandy soil, in open garden, with radicle down, secondary roots often came to the surface, where they perished. When planted with the tips or radicles uppermost, about ten per cent sent the primary root out of the soil.

Lima beans planted with the hilum down all came up well. After some time many seeds of Lima beans with the hilum uppermost sent some portion of the germinating plant out of the ground. Five out of eighty seeds soon perished when planted in the cellar with the hilum uppermost.

In the open garden, nine out of twenty-five sent the roots and lower end of plumule out of the soil, when the seeds were planted with hilum uppermost. These seedlings soon perished.

Observations on the Fertilization of Yucca and on structural and anatomical Peculiarities in Pronuba and Prodoxus.

By C. V. Riley, of Washington, D. C.

[ABSTRACT.]

This paper records some recent experiments and observations which establish fully and conclusively the fact that Pronuba is necessary to the fertilization of the capsular Yuccas. It describes for the first time how the pollen is gathered and collected by the female Pronuba. The act is as deliberate and wonderful as that of pollination. Going to the top of a stamen she stretches her tentacles to the utmost on the opposite side of the anther, presses the head down upon the pollen and scrapes it together by a horizontal motion of her maxillæ. The head is then raised and the front legs are used to shape the grains into a pellet, the tentacles coiling and uncoiling meanwhile. She thus goes from one to another until she has a sufficiency.

My observations confirm the accuracy of Dr. Geo. Engelmann's conclusion as to the impotence of the stigmatic apices in some of the Yuccas and show how the apparently contradictory experience of Mr. Meehan can be reconciled on variation in this respect in the species of the same genus. The exceptional self-fertilization in *Yucca aloifolia* — the only species in which it is recorded — is shown to be due to the fact that in the fruit of this species, there is no style, the stigma being sessile and the nectar abundant, filling and even bulging out of the shallow opening or tube. The flowers are always pendulous and the pollen falling from anthers, under favorable circumstances, readily lodges on the nectar.

The irregularity in the shape of the fruit of the Yuccas — considered a characteristic by botanists — is proved by experiments to be due to the punctures of Pronuba.

The egg of Pronuba which averages 1.5 mm. long, having a swollen apical end and a long and variable pedicel is passed into the ovarian cavity. The puncture is made usually just below the middle of the pistil on the deeper depression which marks the true dissepiment, or through the thinnest part of the wall. The horny part of the ovipositor reaches the longitudinal cavity at the external base of the ovule near the funiculus, without, as a rule, penetrating or touching the ovule itself, and the delicate and extensile oviduct then conveys the egg for some distance (the length of six or eight ovules) along the cavity, the terminal portion of the oviduct being

furnished with retrorse hairs which help to hold it in place during the act.

The paper concludes with some studies of the internal anatomy of Pronuba and Prodoxus.

The Hibernation of Aletia xylina (Say), in the United States a settled fact. By C. V. Riley, of Washington, D. C.

[ABSTRACT.]

I have already shown in previous remarks before the Association that there were various theories held by competent men — both entomologists and planters — as to the hibernation of this Aletia (the common cotton worm of the south); some believing that it hibernated in the chrysalis state, some that it survived in the moth state, while still others contended that it did not hibernate at all in the United States. I have always contended that the moth survived [illegible] in the latitude of the United States, and in this paper the fact of its hibernation, principally under the shelter of rank [illegible] is established from observations and experiments made [illegible]

plant upon which the worm feeds within the cotton belt; so that in the light of the facts presented there is all the greater incentive to that mode of culture which will prevent the growth of rattoon cotton, since it is questionable whether the moth would survive long enough to perpetuate itself upon newly sown cotton except for the intervention of the rattoon cotton.

EMULSIONS OF PETROLEUM AND THEIR VALUE AS INSECTICIDES. By C. V. RILEY, of Washington, D. C.

[ABSTRACT.]

THE value of petroleum for the destruction of insects has long been recognized and I have for years been endeavoring to solve the question of its safe and ready use for this purpose without injury to plants. The paper contains the results of extended experiments carried on under my direction, by several of my assistants and particularly by Prof. W. S. Barnard, Mr. Jos. Voyle of Gainesville, Fla., Mr. Clifford Richardson, assistant chemist of the Department of Agriculture, and Mr. H. G. Hubbard, who has for over a year been devoting his time to practical tests in orange groves at Crescent City, Fla.

Passing over the ordinary methods of oil emulsions by phosphates, lactophosphates and hypophosphites of lime and various mucilaginous substances, experience shows that, for the ordinary practical purposes of the farmer and fruit-grower, soap and milk are among the most available substances for the production of petroleum emulsions.

Ordinary bar soap scraped and rubbed into paste at the rate of 20 parts soap, 10 parts water, 30 parts kerosene and 1 part of fir balsam, will make, when diluted with water, an emulsion stable enough for all practical purposes, as the slight cream which in time rises to the surface, or the flakiness that often follows is easily dissipated by a little shaking. Soap emulsions are, however, less satisfactory and efficient than those made with milk. Emulsions with milk may be made of varying strength, but one of the most satisfactory proportions is 2 parts of refined kerosene to 1 part of sour milk. This must be thoroughly churned (not

merely shaken) until a butter is formed which is thoroughly stable and will keep indefinitely in closed vessels and may be diluted *ad libitum* with water when needed for use. The time required to bring the butter varies with the temperature and both soap and milk emulsions are facilitated by heating the ingredients. Ordinary condensed milk may also be used by thoroughly stirring and beating it in an equal or varying quantity of kerosene.

The diluted emulsion when prepared for use should be finely sprayed upon the insects to be killed, its strength varying for different insects or plants and its effect enhanced when brought forcibly in contact with the insects.

Of mucilaginous substances, that obtained from the root of *Zamia integrifolia*, a plant quite common in parts of Florida, and from the stems of which the Florida arrowroot is obtained, has proved useful as an emulsifier.

These petroleum emulsions have been used with success by Dr. J. C. Neal, of Archer, Fla., against the Cotton Worm, without injury to the plant, but their chief value depends on their efficacy against the different scale-insects which affect Citrus plants. Experience so far shows that such plants do not suffer from its judicious use, but that it must be applied with much more care to most deciduous fruit trees in order not to injure them.

A new Sexual Character in the Pupæ of some Lepidoptera.

By J. A. Lintner, of Albany, N. Y.

[ABSTRACT.]

The sexual character of insects have always been of much interest to entomologists, even when, as in most insects, they are well-marked, and much more so when they are quite obscure and only to be discovered by study and comparison.

Many of the features seem to be directly dependent upon sex, while others apparently serve no special purpose. A number of these are mentioned in the paper.

It would not be expected that sexual features should be as numerous and as marked in the earlier stages of insects as in the final stage. We find them rarely in eggs. Sex clearly exists in

the larval stage, although it is but seldom shown in external characters. The caterpillar of *Thyreus Abbotii* is generally believed to show distinct sexual forms.

I have heretofore pointed out sexual characters in the anal segment of some *Sphingidæ*. In this paper attention is called to a very interesting feature indicating the sexes of the pupæ of the *Cossinæ*, and of such *Ægeridæ* as have been examined. These species, living as larvæ and pupæ in the interior of plants and trees, are provided with a pupal armature of two transverse rows of teeth or spines upon most of the abdominal segments, which serve, at their time of final transformation, to release them from their burrows and partially to extrude them from the trunk.

In the male, the tenth segment (not counting the head as one), or the antepenultimate, *is furnished with two rows of teeth, while the female uniformly has but one* (as have the two following segments in each sex).

Another interesting feature of this armature is, that it discloses excellent characters for the determination of species.

On an Egg-parasite of the Currant Saw-fly, Nematus ventricosus. By J. A. Lintner, of Albany, N. Y.

[ABSTRACT.]

In the twelfth Report of Dr. Fitch on the Insects of New York, he refers to the discovery of this interesting and efficient parasite made by me at Utica, N. Y., in the year 1866. Two years later, at Schoharie, N. Y., I again collected the insect, and by confining individuals in a glass supplied with newly deposited eggs of the currant-fly was able to observe the manner of its oviposition, placing two or three of its eggs within one of the currant-fly.

The specimens then obtained were destroyed before they could be satisfactorily studied and determined. From that time until the present year (1882) no more examples could be found although meanwhile faithfully sought for.

In June last, it was rediscovered in my garden at Albany, N. Y., and from another garden in the same city, a large number of leaves were collected bearing the parasitized eggs of the currant-fly,—

at once recognized by their brown shades in their early stage, and by the pupal form later.

Packages of the egg-bearing leaves (containing from two hundred to three hundred eggs each) were at once made up, and sent to entomological friends in several of the States and Canada, with the request that they be pinned upon currant bushes containing eggs of the currant-fly, in the hope that the valuable parasite might thus be extensively and more rapidly distributed than if left to its natural distribution. Several parasites of other insects have already been distributed in this manner, and it is hoped that the present effort will prove to be successful, and aid in checking the ravages of this well-known and great pest of our gardens.

The specimens of the parasite obtained by me — exceedingly minute insects, barely exceeding the hundredth of an inch in size and belonging to the family of Chalcididæ — prove to have been described by Professor Riley recently, in 1878, as *Trichogramma pretiosa*. It had been obtained by him from eggs of the cotton-worm moth, *Aletia argillacea*, collected in the southern states. Another species of the genus, *T. minuta*, parasitizes the eggs of one of our butterflies, *Limenitis disippus*.

Observations on the Elm-leaf Beetle, Galeruca xanthomelæna. By G. Macloskie, of Princeton, N. J.

[ABSTRACT.]

Its ravages at Princeton 1880–1882. The adult winters in cellars and attics of houses: comes out early in May: ovipositing: form and development of ova. The larva described and its habits. The pupa, its form and habit of lying concealed in crevices of bark and about the base of trees. Coming-out of the beetles: their forms and habits: ¼ inch long, with orange elytra having two longitudinal black bars. Sucker-bristles on tarsus: heart seen beating through scutellum: mode of folding wings (1) longitudinally along main ribs (2) obliquely at the tip (3) obliquely at internal angle near base. The folding effected by the tension of the elastic chitine.

Powers of flight moderate (slow progress in extending to new localities).

Second course of eggs on second crop of elm-leaves. Probably three broods in the season.

Damage to the trees. Efforts to counteract this, (1) by destroying beetles in their winter refuge, (2) by kerosene on the bark of the trees (it kills leaves) when the larvæ are travelling, (3) by collecting and destroying the pupæ while changing, (4) by entrapping the beetles with old carpets, etc., (5) by sprinkling with London purple (1 lb. in 100 gallons of water). This mode is recommended by C. V. Riley.

No advantage from wrapping the stems in tarred cloth nor from leaving lighted lamps near the trees at night.

On the Gall-mites, Phytoptus. By Wm. A. Buckhout, of State College, Pa.

[ABSTRACT.]

In the spring, galls caused by Phytopti may be found upon the leaves of the white maple as soon as they have expanded, and a careful examination shows the galls in process of formation while the leaves are still folded within the bud. The mites are upon the outer, *i. e.*, the under sides of the leaves, which when in the bud are very hairy. The irritation caused by the mites produces an abnormal multiplication of vegetable cells and the gradual arching upward of that part of the leaf, thus making a gall which differs from the galls produced by insects in that it has an opening below, which is frequently lined with hairs. Though generally on the upper surface of the leaf they are sometimes on the lower. These result from stray mites that have worked their way around to the upper side. The gall is always on the side opposite to that upon which the mite begins its attack. Each gall contains at first but one mite. The galls enlarge with the leaves and white globular eggs appear. A gall eventually becomes tenanted by a large number of mites. These migrate to all parts of the tree. Some produce (generally on the under side of the leaf) the growth of hypertrophied hairs called Erineum. The mites live and

breed in this growth as they do in the galls. Later they seek winter quarters, and may be found in the crevices of the bark in the maple, and between the scales of the buds in the pear. The hairiness of the bud scales of many trees makes detection of such small objects as gall-mites very difficult.

The gall-mites when fully grown are of a narrow cylindrical form and average $\frac{1}{4}$ to $\frac{1}{3}$ mm. long. All variations in size can be accounted for by difference in the age of different individuals, and by the ease with which they change form. There are two parts to the body, cephalothorax and abdomen; the latter is minutely striated, the former smooth, and bears the legs and mouth appendages.

The mouth parts are a pair of blunt pointed mandibles held closely together, between which is a blunt body, perhaps the maxillæ, generally retracted, but occasionally protruded.

Eyes are wanting. The legs are four, placed far forward, close together, and alike in size and shape. Each leg is five-jointed, the second joint the longest. There are one or two curving bristles at certain joints. The terminal joint is made up of a finger-shaped process to which is opposed a feather-like body of extreme tenuity having a central rachis and five barbs.

The abdomen comprises seven-eighths of the body, is closely striated, and very uniform except at the free extremity which is smooth and sucker-like. There are seven pairs of bristles attached to the body; one pair at the base of the second pair of legs, one pair on the back anteriorly, two pairs on the side of the abdomen anteriorly (but at slightly different planes), one pair on the side of the abdomen posteriorly, one pair long, and one pair short, anal bristles; each bristle grows from a small tubercle.

There is a large clear space in the anterior part of the body cavity of an adult and behind it are two great masses of eggs, lying close to the abdominal walls, leaving a more or less clear central channel. In its hermaphrodite character the resemblance of Phytoptus to the Tardigrades is very marked, although the testes have not been made out. In external form there is resemblance to Demodex and Pentastoma.

The gall mites walk with considerable ease using the legs as an ordinary quadruped does.

The gall mites are adult, and have no further change.

In all the galls and abnormal growths examined by me, the

mites were essentially the same, and no difference worthy of being ranked specific can be found.

The blister spots of the pear and the malformed flower clusters of the black ash, while not technically galls, are abnormal growths caused by peculiarities resident in the plants themselves, and not in the mites.

The cases in which gall mites were found upon berries of the grape where they bred in the minute roughened and cracked spots in the pedicels, no galls, erineum or malformation being produced anywhere in the vicinity, make plain the fact that galls are not the necessary sequence of the presence of these mites, but that so-called gall-mites are widely scattered over the cellular tissue of plants in which they may produce malformations of various kinds, or none at all, according to circumstances.

The following is a list of the trees and plants on which mite-galls and other malformations caused by Phytopti have been found by me.

1. White Maple (*Acer dasycarpum*)
   a. galls, irregularly rounded, inner wall roughened by protuberances, medium sized (3–5 mm.)
   b. erineum, crimson, under side of leaf.
2. Red Maple (*Acer rubrum*)
   a. galls, reddish, rounded, small (1–2 mm.)
   b. erineum, crimson, under side of leaf.
3. *Sugar Maple* (*Acer saccharinum*)
   galls elongated (3–5 mm. long), pointed, interior roughened.
4. Black Cherry (*Prunus serotina*)
   galls, large (5–8 mm. long), obovate, tapering to a narrow stem, generally curved, and splitting lengthwise.
5. Wild Yellow Plum (*Prunus Americana*)
   galls as in *P. serotina* but smaller, more slender.
6. Cultivated Pear (*Pyrus communis*)
   "Blister spots," at length blackened.
7. Linden (*Tilia heterophylla*)
   galls, elongated, pointed (3–5 mm.) interior densely covered by long, curved unicellular hairs.

8. Black Ash (*Fraxinus sambucifolia*)
   flower clusters, malformed, producing a cauliflower-like mass.
9. Willow (*Salix*———sp.)
   galls, small, numerous, clustered, reddish.
10. American Beech (*Fagus ferruginea*)
   erineum, yellow, upper side of leaf (but one specimen examined).
11. Poison Ivy (*Rhus toxicodendron*)
   galls, small, numerous, clustered.
12. Button Bush (*Cephalanthus occidentalis*)
   galls, small, numerous, clustered along the veins.
13. Sour Gum (*Nyssa multiflora*)
   galls, small, numerous, clustered, becoming corky.
14. Yellow Birch (*Betula lutea*)
   erineum, reddish green, upper side of leaf (but few specimens found).
15. Red Birch (*Betula nigra*)
   galls, small, numerous, rounded, interior smooth or slightly granular.
16. Speckled Alder (*Alnus incana*)
   galls, small, rounded, reddish.
17. American Elm (*Ulmus americana*)
   galls [illegible] interior filled with large [illegible]
18. Cultivated Grape (*Vitis*———sp.)
   slight protuberances of the petioles of the berries [illegible]
19. [illegible]
   [illegible]
20. European Alder (*Alnus glutinosa*)
   [illegible]

All species were found [illegible] by Franklin B. Hough.

On the Mouth of the Larva of Chrysopa. By Wm. Saunders, of London, Ont., Canada.

[ABSTRACT.]

The author refers to the form and tubular character of the jaws, the manner in which the chrysopa larva seizes and holds its victims, the aphides, and its method of feeding as seen under the microscope.

At the base of each of the mandibles the integuments are dilated into a sac-like form capable of expansion and compression at will, a portion of the thorax is similarly constructed and it is by the repeated dilating and compressing of these sacs that the fluid contents of the body of an aphis is transferred through the tubular mandibles to the stomach of the chrysopa larva.

The Classification of the Ungulata. By Edward D. Cope, of Philadelphia, Pa.

[ABSTRACT.]

The name Ungulata was used in this paper for the placental mammalia with enamelled teeth, which are furnished with hoofs. Thus defined, it includes the existing groups Artiodactyla, Perissodactyla, Proboscidia and Hyracoidea; and the Amblypoda, Toxodontia and Condylarthra of extinct types. The material of the latter groups now known enables us to understand some of the primary characters of the orders and to perceive the reason of the order of their appearance in time.

Especial attention was devoted to the arrangement of the bones of the carpus and tarsus. Two types of carpus can be recognized. In the first, the trapezoides supports the scaphoid exclusively; in the second, it supports the lunar also. In the first, the magnum supports the lunar, in the second it supports a part of the cuneiform also. To the former type belong the Condylarthra, the Proboscidia, the Amblypoda and the Hyracoidea; to the latter the Perissodactyla and the Artiodactyla.

In the tarsus, three divisions are also easily distinguished. In the first the astragalus articulates only with the navicular, and the

cuboid with the calcaneum only. This includes the Condylarthra and the Hyracoidea. In the second, the astragalus unites with the navicular only, and the cuboid with the navicular and the calcaneum. This includes only the Proboscidians. In the third division, the astragalus unites with both cuboid and navicular, and the cuboid with the astragalus and cuboid. Here come the Amblypoda, the Perrissodactyla, and the Artiodactyla. Collating the relations of both carpus and tarsus, we have then four divisions, each characterized by peculiarities as above indicated. The nomenclature of these divisions it was proposed should be as follows:

Order I. TAXEOPODA; suborders *Hyracoidea* and *Condylarthra*.
Order II. PROBOSCIDIA.
Order III. AMBLYPODA; suborders *Taligrada*, *Pantodonta* and *Dinocerata*.
Order IV. DIPLARTHRA; suborders *Perissodactyla* and *Artiodactyla*.

The Amblypoda is the only one of these orders which has no recent representatives. The Condylarthrous division of the Taxeopoda is the oldest group.

The order of evolution has been as follows: in the Taxeopoda the relations of the carpal and tarsal bones are in separate continuous series. In the Proboscidia and Amblypoda, the carpal series is linear, and the tarsal series alternating, but in different ways. In the Diplarthra the carpal and tarsal bones alternate with each other in the longitudinal direction, or form oblique series. It is evident that the alternating series form the most compact and effective structure, resisting strains much more strongly than that of the order Taxeopoda, or the linear arrangement. In the Proboscidia, we have the weaker structure of the forefoot, and a peculiar but stronger one of the posterior foot. In the Amblypoda we have partly the same structure of the forefoot, while the posterior foot has the interlocking character of the hinder foot of the Diplarthra.

Three points are here to be observed. I. That the weaker structure of the carpus and tarsus appears first in time. II. That the stronger structure appeared first in the posterior limbs. III. That the interlocking structure has greatly multiplied, while the linear has dwindled and mostly disappeared. Here is a direct connection between mechanical excellence and survival.

The appearance of the alternating type first in the posterior foot, is a fact parallel to the reduction of the toes in the hind foot earlier in time than in the fore foot. This I have suggested has been due to the greater force of the impacts and strains of the hind foot than the fore foot, since it is the limb which propels the body, while the anterior limb principally supports. The reduction of both fingers and toes has commenced on the inner side, and the second row of carpal bones has coincidentally with this event, been rotated inwards, with the obvious effect of giving a more alternating position to the digits. The change in the tarsus has been effected by the inward extension of the cuboid, which also gives greater support to the external, and subsequently to the penultimate toe.

THE FAUNA OF THE PUERCO EOCENE. By EDWARD D. COPE, of Philadelphia, Pa.

[ABSTRACT.]

THE paper on the fauna of the Puerco Eocene epoch included a general account of fifty species, mostly mammalia, of the oldest tertiary formation known. The author said he discovered the formation in 1874, in N. W. New Mexico, and that in 1876 it had been observed by Endlich and Holmes in southern Colorado. Its deposits reach a depth of 1000 feet, and consist of marls divided by beds of sandstone.

Of Reptilia, six species only had been found, one tortoise, one crocodile, and three species of Champsosaurus. The latter genus forms a peculiar suborder probably of Lacertilia (lizards), and is characteristic of the underlying Laramie or Cretaceous No. 6 of America, and has been obtained from the lowest Eocene of Reims, France. The species were aquatic, according to Lemoine.

The Mammalia present many peculiarities. The Rodentia, Carnivora, and all the existing orders of Ungulata are wanting, and their place is taken by Marsupialia, Bunotheria and the ungulate suborders Condylarthra and Taligrada.

Five species of Marsupials have been found, which are partly related to the Plagiaulax of the Jurassic, to the Pliocene Thylacoleo, and existing kangaroos of Australia. The most remarkable are the two species of Ptilodus, a genus which has also been found in France with the Champosaurus. *Catopsalis pollux* was larger than any existing kangaroo.

Of the supposed Bunotheria, some may be Marsupialia. They include two species of lemuroids, seven species of Creodonta (flesh-eaters) two of Tillodonta, one of Tæniodonta and two of Insectivora. The lemuroids (*Adapidæ*) belong to genera of the Wasatch epoch, while of the creodont genera, only one (*Didymictis*) belongs to that epoch, and three are peculiar. One of these is Sarcothraustes, which represents the flat-clawed group of the Wasatch and included a large powerful species (*S. antiquus*) as large as a black bear. Deltatherium and Triïsodon are very opossum-like Creodonta. The Tillodonta belong to the genus Psittacotherium, which is provided with four powerful cutting rodent-like incisors in the lower jaw. It was a nutcracker or bark stripper. Of the insectivora, Conoryctes with two species is the peculiar genus. It has well developed canines, and probably lived on hard substances.

The suborder Condylarthra is found only in this formation and in the Wasatch, and is represented in the Puerco by two families, the Phenacodontidæ and Meniscotheriidæ and twenty-five species. The former family has simply tubercular, the latter crested teeth. The suborder is more nearly allied to the Hyracoidea (conies) of Asia and Africa, than to any other, and with that and probably some other groups should be combined in a special order, the Taxeopoda, which differs alike from its nearest allies, the Amblypoda and the Proboscidia. Besides Meniscotherium and Phenacodus, there have been referred to this order six genera with eleven species, which are only known from jaws and teeth. Some of these have some of the cusps of the inferior molars compressed, so as to serve the sectorial function of a flesh-eater. This is especially the case with *Haploconus*. Periptychus was at first supposed to be a creodont. It has the teeth remarkably sculptured. *Pantolambda bathmodon*, the size of a sheep, is the largest species, and is an aberrant Amblypod.

In review, it appears that of the fifty known species of the Puerco epoch, all are peculiar to the formation. Three belong to a genus of the Laramie epoch. Ten belong to six genera which are also found in the Wasatch epoch. One (*Hyopsodus*) ranges to the Bridger. Of the sub-orders only two (*Marsupalia*, *Insectivora*) still exist, and three, including eighteen species, are not found above the Eocene. Two other Eocene sub-orders are represented by a very few species in the lowest Miocene.

On a Recent Species of Heteropora from the Strait of Juan de Fuca. By J. F. Whiteaves, of Ottawa, Ontario.

The genus *Heteropora* was constituted by De Blainville in 1830, for the reception of certain erect and more or less branching Polyzoa, whose calcareous skeleton is composed essentially of slender tubes of comparatively large size, each of which is surrounded by a single row of smaller ones. The larger tubes, which are usually circular in section, are universally admitted to represent the polyp-cells proper, — but the functions of the smaller ones, which Mr. Busk calls the interstitial canals and which are often angular in section, are by no means clearly understood. It has been ascertained, too, that the smaller tubes at least are provided with cross partitions, that both kinds are furnished with radiating spines, and that the walls of both are perforated by numerous openings. These cross partitions and radiating spines have been held to be the homologues of the tabulæ and septa of the tabulate corals and the perforations in the walls of the tubes as analogous to the mural pores of the Favositidæ.

Lindstrom, in 1876, maintained that the Palæozoic fossils known to geologists under the names *Chætetes*, *Stenopora and Monticulipora* have almost exactly the same kind of internal structure as *Heteropora*, and that the former genera therefore should be removed from the class to which the true anthozoan corals belong and placed in that of the Polyzoa, a conclusion which had been arrived at ten years before by Dr. Rominger.

Many species of *Heteropora* have been described from the Mesozoic and Tertiary rocks of Europe and the United States, but no living representatives of the genus had been discovered until 1879. In that year Mr. Waters described and figured a recent species from Japan, under the name *H. pelliculata*, in the Journal of the Royal Microscopical Society, and a little later in the same year Mr. Busk published a diagnosis, with illustrations, of a living polyzoon from New Zealand, which he called *H. Neozelanica*, in the Journal of the Linnean Society. Mr. Waters and Dr. H. A. Nicholson, however, are both of the opinion that the *H. Neozelanica* is identical with the previously described *H. pelliculata*.

On the coast west of Sooke, Vancouver Island, in the Strait of Juan de Fuca, Mr. James Richardson, late of the Geological Survey of Canada, found a single specimen of a recent calcareous polyzoon,

in 1874, which, in the writer's judgment, cannot be distinguished by any tangible character from the Japanese and New Zealand species of *Heteropora* described by Messrs. Waters and Busk. No thin sections of this specimen have been made to show the minute structure of the interior, but the whole of the outer surface has been carefully examined under the microscope and camera drawings have been made of some of the appearances thus presented. The punctured calcareous pellicle which Mr. Waters represents in his figures as closing the mouths of the interstitial canals in *H. pelliculata*, and to which the specific name refers, can be well seen in the Canadian specimen. On other portions of the latter the apertures of the larger tubes project distinctly beyond the general level of the surface, a feature which is not indicated on any of Messrs. Waters' or Busk's illustrations, but this slight variation from their types can scarcely be held as indicating a specific divergence or difference from them.

On the Habits of Cryptobranchus. By Burt G. Wilder, of Ithaca, N. Y.

[ABSTRACT.]

Living examples of Cryptobranchus have rarely been brought to this country, and the following observations may be worth recording even if they merely confirm those of Dutch and German writers, whose works I have not yet been able to consult.

This specimen is about 75 cm. (two and one-half feet) long, and was obtained for Cornell University through Prof. H. A. Ward, who brought it by hand from Japan.

It is very sluggish, remaining quiet for hours in the water, excepting for the periodical respiratory movements presently to be described. Nevertheless, it can display considerable activity, and on one occasion escaped from a common wash-tub which was about 30 cm. (one foot) deep. Out of water it appears uncomfortable and crawls first in one direction and then in another, with frequent stoppages. It evidently seeks the shade, but gives no sign of discriminating objects, walking against dogs and cats and people as against wood or stone. The trunk is never lifted from the ground, and the compressed tail rests upon one side, but the head and neck are sometimes raised.

The respiratory actions in deep water I have not observed

accurately; in shallow water just covering it, the nostrils are raised above the surface at frequent intervals, a hissing sound is heard, and after the nostrils are again carried below the surface, a few bubbles of air escape therefrom, and there are muscular movements about the neck. During an hour, in freshly changed water, these respiratory motions occurred at intervals varying from half a minute to twelve minutes, but usually the time was from two to four minutes.

No notice is taken of raw or cooked beef or fish, either floating at the surface, lying at the bottom or suspended just above the water. But if bits of food are dropped close to the mouth, or allowed to slide over the top of the head or held at the lips they are readily snapped up and swallowed if not too large. After a time, the head was moved slowly toward meat held about 1 cm. from the lips; I could not determine whether sight or smell were the sense concerned. Neither have I ascertained the functions of the tubercles.

This specimen has now eaten the following: beef heart, raw and boiled; blue-fish, raw and boiled; hard boiled white of egg; canned roast beef; raw lambs' liver, heart, diaphragm, lung and thymus; baked maccaroni.[1]

THE PLACENTAL DEVELOPMENT IN MAMMALS: A UNITY OF ANATOMICAL AND PHYSIOLOGICAL MODALITY IN ALL VERTEBRATES.

By HENRY O. MARCY, of Boston, Mass.

[ABSTRACT.]

RECENT investigations by a number of distinguished scholars have given this subject a new and exceptional interest. Deductions have been formulated of fundamental importance not only to the physician and the pathologist, but also to the comparative anatomist and scientist. The text-books repeat almost unchanged the teachings of twenty years ago. The value of comparative an-

[1] According to Prof. D. S. Jordan, the prior generic name of the great Japanese salamander is *Megalobatrachus*. *Menopoma* should be replaced by *Cryptobranchus* and *Menobranchus* by *Necturus*.

The subject of this abstract died May 25, 1883. Beside the skin and skeleton, preparations of the brain, heart and other viscera are preserved in the museum of Cornell University.

atomy in clearing up intricate questions pertaining to the structures of the human species has been generally underestimated; especially is this true in reference to the human placenta. The older masters of the Italian schools (Phillipo Ingrassius, Fabricius, Malphigi) studied with great care many phases of placental development in mammals. The modern investigations of Owen, Huxley, Turner and especially of Prof. Ercolani of Bologna are of the first importance.

The simplest form of placental development is found in the diffused placenta of the solipeds.

In the non-gravid uterus of the mare, "the mucous membrane is smooth and velvety, of a rose color inclining to yellowish, and here and there it is doubled over into large and soft folds. In the gravid uterus, on the contrary, the folds are wanting and over the whole surface of the mucous membrane are scattered vermiform projections winding closely against each other, of a very intense deep red color tending to violet, and the surface has a swollen appearance."[1]

Vertical sections of the uterus, in an advanced stage of gestation, show in the place of the mucous membrane, which in the non-pregnant state is scarcely distinguishable, a uniform layer from one to two millimetres thick, which upon superficial examination appears like a transformation of the uterine mucous membrane. Into this the villi of the chorion enter and are somewhat uniformly distributed over the whole uterine surface. "This so-called tumefied mucous membrane has its origin from the sub-epithelial connective tissue and consists of an infinite number of simple glandular follicles, and may very properly, as designated by Ercolani, be considered as a new glandular secretive organ. The villi of the chorion are enclosed by these introflexions and follicular depressions, instead of entering as was earlier supposed the uterine utricular glands. These glandular follicles are supplied with an internal epithelium and are developed only during gestation.

The careful study of the multiple placenta of the Ruminants teaches much of value. Fabricius thought there was a direct communication between the vessels of the mother and the fœtus. Harvey, on the contrary, maintained that the fluid secreted by the cotyledons was absorbed by the villi. This view was sustained by Needham and Haller.

[1] Ercolani, Utricular Glands of the Uterus, etc., page 35.

Ercolani has demonstrated that upon the site of the rudimentary cotyledons, which are never wanting in the non-pregnant state, there is developed during pregnancy a new organ of glandular secretory character and that this differs from the diffuse placenta chiefly in being developed over circumscribed areas and in possessing a more complex condition.

The study of the single placenta is beset with far greater difficulties. A transverse section of a gravid uterus of the rabbit at the placental site, the second week of pregnancy, shows three parts each clearly distinct.

1. The muscular wall of the uterus.

2. The placental neoplasm which is developed upon the inner uterine surface.

3. Above this is the old uterine mucous membrane tumefied and about to undergo destructive changes.

Two most noteworthy conditions appear.

The utero-placental vessels have a lumen almost double the vessels from which they proceed, and notwithstanding their greater volume they show in their walls none of the anatomical characteristics, which serve to distinguish the arteries from the veins and which are clearly visible in the uterine vessels.

The second and even more important fact is that these vessels, instead of the ordinary walls, are surrounded with a uniform envelope of cells of a special character, which cannot be distinguished from the cells of the decidua serotina and the maternal placental tissue. The special characteristic of the walls of the vessels, composed as they are seen to be of only the endothelium, is an objection to the supposition that their development could proceed from the vascular network of the old transformed mucous membrane. This is emphasized by the exceptional fact that the old uterine mucous membrane is detached from the muscular wall, and with its glands is undergoing a process of rapid destruction. These facts place it beyond all doubt that the utero-placental vessels and their surrounding cells are the result of a real neo-formative process.

In all animals having a single placenta, the decidua vera and the decidua reflexa are of new and distinct formative character. The decidua vera is composed of newly formed cells which are early arrested in their development and superimposed upon it is

the old uterine mucous membrane, usually easily distinguished by its glands, and which later is seen in process of destruction and interblending between the external layer of the ovum and the internal uterine wall.

In most animals the newly formed placental vessels have a uniform diameter and constitute a network of small meshes which reaches as far as and is firmly joined to the chorion.

In the meshes of this network, are found the fœtal vessels which unite and enter the cord. By injection of these we have a better demonstration of the relations established between the fœtal and maternal portion of the placenta.

A careful study affords convincing proof that the umbilical arteries ramify and terminate in a thin fine network of capillaries in the chorial villi and are in close contact with the cellular covering which clothes the maternal vessels. Demonstrative evidence of this relationship in the human placenta has been frequently observed by myself, where the placenta has been undisturbed in its connections with the uterus for a considerable period after the death of the fœtus.

The chorial villi become shrunken and easily separate from the decidual sheath, the cells of which remain unchanged.

In certain of the lower animals (*e. g.*, the *Cavia cobaya* or guinea pig) Ercolani has observed that the maternal vessels become dilated in their terminal loops. This is of especial importance since it furnishes an example in rudimentary condition, of that which takes place in an extreme degree in the human placenta.

The destructive changes in the uterine mucous membrane of woman after conception are limited to the epithelial layer. After the separation of the decidua vera from the internal uterine surface, it is observed that the muscular wall is covered with a mucous membrane identical with that lining the uterine cavity before impregnation. The uterine mucous membrane is little more than a simple layer of epithelium and the reproduction of this is all that is required to restore the internal surface of the uterus to its primary state. During the early period of development of the human placenta, the decidual cells form a richly vascular compact layer surrounding the chorial villi. For a limited time the relationship between the parts is not so intimate that they

cannot be separated. At this stage, before the maternal vessels become ectasic, the form of development is not unlike the diffused or disseminated placenta.

The walls of the proliferating branches of the villi are necessarily forced against the decidual layer of cell covering the ectasic, thin walled maternal vessel. Thus this is introflected and a close union ensues. In this manner the so-called placental lacunæ are formed in the fully developed placenta, and, from this very deceptive appearance, it has been held as a demonstrated fact, that the fœtal villi swim in the maternal blood.

The epithelial covering of the vascular loop of the fœtal villus is wanting because of the intimate relation which has been established between it and the secretory villus, which *never* loses its own epithelium. The form of the vascular loop of the maternal villus varies greatly. In the quadrumania and in woman the dilatations become lacunose and are actually enormous.

All the vertebrates during the period of embryonic life, in order to complete the marvellous phases of their development, require a special nourishment which is always furnished them by the mother. This is conveyed to and converted into its own substance by the embryo, and however many and considerable may be the differences met with, in these two fundamental factors, the unity running through them readily appears under two general forms, represented by the yolk of the egg in oviparous animals and by the placenta in mammals. In the first, the maternal aliment is stored up in a mass by the mother and emitted with the ovum in the quantity needed by the embryo to complete its development.

In the second, the nutritive material is furnished by the mother from the placenta which elaborates it as needful for the developing embryo. These materials in every case are absorbed and conveyed to the fœtus by means of its own vascular appendages. Thus nutrition is carried on during the development period in all vertebrates by a single law of physiological modality.

The typical anatomical form in the two fundamental parts of the placenta, however wide the variation, is always identical:—a villus of new formation maternal as well as fœtal, each composed of an internal vascular loop, surrounded by a cellular layer and covered with an outer epithelium. The office only of the villi is different, the fœtal *absorptive*, the maternal *secretive*. These villi always come in contact more or less intimate, but the walls of

the vascular loops never in any instance touch. This is the more to be emphasized since it invalidates the idea hitherto held, and yet almost universally taught, that the mode of the nutrition of the fœtus is an interchange between the two bloods by endosmosis and exosmosis. The different ways of the interblending of the fœtal and maternal villi give the complex and confusing picture of placental formation. The belief that the lacunæ were really large cavities as they certainly appeared, and not the maternal vessels greatly dilated, was held as a truth proved and indisputable, and it was through this belief that two other deceptive appearances were received as actual truths, namely, that the villi floated in the maternal blood and that the epithelium covering them appertained to the fœtus instead of the mother.

THE MORPHOLOGY OF ARTERIES. By FRANK BAKER, of Washington, D. C.

[ABSTRACT.]

THE arterial system is remarkable for irregularity and variability; yet an attempt should be made to bring it under morphological laws.

Ontogenetic development shows that the embryo has at first no vessels. These arise by vacuolation of cells which unite to form capillary tubes commencing at the periphery of the area vasculosa and approaching the heart. Uniting, these form trunks along lines of axial growth. This *centripetal* development is to be considered when attempting to interpret the laws which govern the arterial system.

Phylogenetic development confirms and supplements this evidence. The lowest animals possess no proper vessels, but obtain food by protoplasmic imbibition. The alimentary canal may ramify in the sarcode, curiously resembling a vascular system, but analogous only. The water-vascular system formed by a split between the two layers of the body wall is possibly the origin of the lymphatics. The blood-vascular system commences by longitudinal trunks. The movements of the body being more frequent in the direction of the mouth tend to develop such trunks. In

higher forms these are supplemented by transverse trunks corresponding to the somatomes. In animals having limbs the limb-trunks show a law of uniform dichotomous division relating to the segments of the limb. If we admit Gegenbaur's theory that limbs arise as buds upon which other buds form in a radiate manner, the arterial ramification can be explained. Some of the arterial trunks belong to aborted rays. When arteries do not divide at the normal point they have a tendency to do so at the node next nearest the heart. This is caused by variability of convergence. One artery may take the place of another by inter-substitution. Functional activity may greatly modify the vascular system. In the intestinal canal (splanchno-pleure) protected from external influences, the cell-life is simple and there has been less differentiation than at the periphery (somato-pleure). The primitive type is more nearly preserved there, and the vessels resemble a capillary plexus vastly enlarged.

The phenomena of ramification can be explained by reference to an ancestral archetype from which the arterial systems of all vertebrates are derived.

The paper was profusely illustrated by drawings and diagrams.

ILLUSIONS OF MOTION. By H. P. BOWDITCH and G. STANLEY HALL, of Boston, Mass.

[ABSTRACT.]

I. IT is a very old observation that when the eye has rested for a time upon a moving object or a series of moving objects and is then transferred to a stationary object the latter seems for a few moments to be animated by a movement in the opposite direction.

Examples of this phenomenon are furnished by watching a waterfall and then looking at the stationary bank, or by looking at the floor of a moving railway car after watching the apparent motion of the landscape without.

The term "subjective complementary motion" has been applied to illusions of this sort on account of the analogy which they present to complementary after images. Prof. S. P. Thompson who

suggests the use of this term formulates the following law: "The retina ceases to perceive as motion a steady succession of images that pass over a particular region for a sufficient time to induce fatigue, and on a portion of the retina so affected the image of a body not in motion appears by contrast to be moving in a complementary direction." (See Quarterly Journal of Science, March, 1879.)

The observation of a revolving disk on which a spiral line is drawn affords perhaps the readiest means of studying this illusion, at the same time that it shows the inadequacy of the explanations which have been hitherto offered. If the disk be revolved in such a way as to produce a contraction of the spiral line towards the centre, an apparent expansion is observed when the gaze is directed to a stationary object. Now, if the stationary object thus viewed be a sheet of paper on which a straight line is distinctly ruled, and the eye be directed to a point near but not upon this line, the surface of the paper appears to be animated with a movement of expansion from the point looked at, while the line does not appear to alter its straight direction. In other words, the field of vision as a whole appears to expand, while the separate points in it do not alter their relative position in regard to each other. This is best explained on the supposition that the apparent expansion is a phenomenon connected with the visual centres in the brain, and not with the optical or mechanical apparatus of the eye.

II. The apparent rotation of a set of concentric circles when a "rinsing" movement is imparted to them, as described by Thompson, may be partly accounted for as follows. When a set of concentric circles is moved to and fro across the field of vision, that portion of the circles in which the direction is the same as that of the motion will be mostly distinctly visible. The blurred figure of the circles will thus be divided by two narrow sectors in which the lines will be distinctly seen. Now, when a "rinsing" movement is given to the circles, the direction of the movement is continually changing, and consequently the narrow sectors in which the lines are distinctly seen will appear to move around the circles and thus cause the appearance of rotation. Another factor in the production of this phenomenon is the apparent rotation round a point in its circumference which is produced whenever a circle is carried with a rinsing movement through a circle equal to itself.

III. The apparent slow rotation of a circle with internal cogs in the direction opposite to that of the "rinsing" movement, a phenomenon also described by Thompson, appears to be due to the fact that the end of the cog is most distinctly seen when it is in that portion of its course which is most distant from the line from which it arises, for here it is least obscured by after-images of the line. The phenomenon can be equally well observed when two horizontal lines with cogs directed towards each other are subjected to a rinsing movement, the two lines of cogs appearing to move in opposite directions. These experiments will be reported in full in an article which will shortly appear in the Journal of Physiology. (See Journal of Phys. III, 297.)

Demonstration of a Series of Brains prepared by Giacomini's Method. By William Osler, of Montreal, Canada.

[ABSTRACT.]

The brain (human), as fresh as possible, is placed in saturated zinc chloride solution, in which it floats, so that it has to be turned or covered with cotton wool soaked in the fluid. On the second day the membranes are removed. This is the only tedious part of the process, as the dissection must be carefully made about the nerves. After having been in the solution for about a week, the brain is transferred to alcohol (methyated spirits) in which it sinks so that cotton wool must be placed beneath to obviate the effects of pressure. The organ can remain in spirit an indefinite time, but after six or seven days is ready for the third stage of the process,—immersion in glycerine (best), in which it floats. It remains in this until it sinks to the level of the fluid which usually takes a week. It is then removed, the surplus glycerine washed off and the organ exposed to the air in a protected place. When dry it receives a coating of ordinary varnish and remains unchanged for years. The method is applicable to the brains of small animals, and the stages of the process are shorter.

Cross Heredity from Sex to Sex. By Mrs. A. B. Blackwell, of Somerville, N. J.

[ABSTRACT.]

There is much evidence that each organic cell and organism assimilates food, builds structure, and performs functions under the same law of equal action and reaction that rules inorganic nature and maintains the balance of work among correlated energies. Thus, each higher organism is differentiated to perpetuate, not another organism like itself, but incomplete elements, the incident forces, which, coöperating with complementary elements, can establish the new organism. Hence a permanent moving equilibrium of male and female influence in the new being; and, by the law of averages, each determines the sex in about equal numerical proportions. There is also a well known family resemblance among brothers and sisters which underlies even unlike physical details and various mental ability, and is often an obvious balance of the dissimilar characters of the parents. Mixed races are the conspicuous blending of both types of progenitors.

But, since special characters are entailed to the same sex exclusively, then a corresponding inheritance of more general characters should be entailed to the opposite sex distinctively, if the balance is maintained.

Several classes of facts were given by the writer in evidence of cross inheritance.

With feeble mothers and strong fathers, daughters, at birth, have an apparent advantage over their brothers; but if the inequality is reversed in the parents, the sons begin life better endowed than their sisters.

If parents are strongly marked contrasts, the sons most often resemble the mother and the daughters the father. When this is not readily evident, intimate acquaintance will often bring out cross inheritance in some other form of compensation.

Idiosyncrasies and traits independent of sex are most often reproduced by cross heredity.

In mixed races, Indian and negro, white blood is presumably a male inheritance, and the women as a class are certainly lighter and more Caucasian in features than the men. But in special cases, where the mother is white, the male children more often

resemble the whites. The same law holds in all mixed nationalities to a noticeable extent.

There are complications arising from the general blending of many characters, and therefore room for difference of conclusion; yet, if attention can be definitely called to the facts, it is believed that the compensative law of cross inheritance, from sex to sex, may be definitely established.

THE ORGANIC COMPOUNDS IN THEIR RELATIONS TO LIFE. By LESTER F. WARD, of Washington, D. C.[1]

[ABSTRACT.]

ORGANIC compounds may be defined as "substances whose highly complex and very unstable molecules are composed of those of inorganic compounds or of organic compounds of lower organization, formed on the cooled surfaces of fully developed plants at life-supporting temperatures."

So far as the cosmical origin of the various substances composing the earth's crust, whether elemental or compound, is understood, it seems to have conformed to the following law: the molecules constituting each progressively more complex unit exhibit *increase of mass* accompanied by *decrease of stability*.

The artificial synthesis of organic compounds has obliterated the line formerly supposed to exist between the chemical constitution of inorganic and of organic compounds.

The properties of substances depend upon their molecular constitution: the more complex their constitution the more active their properties. Thus the properties of compounds are as a rule more active than those of elements, those of organic are more active than those of inorganic compounds, those of the alkaloids more active than those of the amyloids, and those of the albuminoids more active than those of the alkaloids. In the last case, however, the activity manifests itself in a different manner, viz., through rapid changes of internal structure including the phe-

[1] This paper was published in full in the American Naturalist, Vol. xvi (December, 1882) pp. 968-979.

nomena of isomerism; thus partially ceasing to be molecular and in a certain way affecting the mass.

These nitrogenous compounds themselves possess grades of complexity and instability, and thus we arrive in an ascending series at proteine and finally at protoplasm.

Protoplasm is a chemical substance whose relatively large molecules may have been compounded of those of the less complex albuminoids.

The activities manifested by protoplasm consist in actual alterations in the mass of the substance, which is the fundamental fact involved in the phenomena of life. Life is therefore essentially a property of protoplasm.

All the higher manifestations of life are reducible to protoplasmic activity, and the enlarged scale on which they operate is due to subsequent *organization*. A living being is only a quantity of organized protoplasm, and tissues are merely the framework and material machinery by means of which the life-substance is enabled to multiply effects.

CLASSIFICATION OF ORGANISMS. By LESTER F. WARD, of Washington, D. C.[1]

[ABSTRACT.]

THE terms *animal* and *vegetable* have proved wholly inadequate to express the distinctions which are found to exist among organisms. As popular terms they are useful, but as scientific terms they have already led to much fruitless discussion.

The fundamental distinction in biology should be drawn between those organisms which are capable of assimilating chemical or inorganic matter, and those which depend entirely upon the appropriation of matter already so manufactured. We would thus have two classes of organisms, viz., first, assimilators, tissue manufacturers, or *autogens;* and second, *parasites.*

The first of these classes might be subdivided into three groups: 1. Those that manufacture protoplasm only and con-

[1] This paper was condensed from the writer's work "Dynamic Sociology" (New York, D. Appleton & Co., 1883, Vol. i, pp. 347–355), published since the Montreal Meeting.

sist entirely of that substance. 2. Those that manufacture both protoplasm and also some form of protective integument or framework. 3. Those which decompose carbonic dioxide and employ the carbon thus liberated as the strengthening material of their tissues, viz., plants proper.

The second class is also divisible into three groups: 1. Those which appropriate matters already manufactured by organisms of the first class (or, as in the case of lichens, take them at third hand from organisms of the same class), but which are not only fixed like plants but are of low organization and simple cellular structure. 2. True parasitic plants, whose organization plainly indicates that they have descended from chlorophyl-bearing plants, of which they are degraded types. 3. Animals proper, which live on the substance manufactured by the first class of organisms, either by taking it directly from plants, or by preying on others of their own group which have derived their sustenance from plants.

TITLES OF OTHER PAPERS READ IN SECTION F.

NOTE ON THE OCCURRENCE OF TRACES OF A NORTHERN FLORA IN SOUTHWESTERN OHIO. By E. W. Claypole of Yellow Springs, Ohio.

NOTE ON THE STERILITY OF THE CANADA THISTLE AT YELLOW SPRINGS, OHIO. By E. W. Claypole of Yellow Springs, Ohio.

INSECTS VERSUS FLOWERS IN THE MATTER OF FERTILIZATION. By E. W. Claypole of Yellow Springs, Ohio.

A SKETCH OF THE HISTORY OF OUR KNOWLEDGE OF THE BUDDING OF SALPA. By W. K. Brooks of Baltimore, Md.

FRITZ MÜLLER AND THE NAUPLIUS OF DECAPODS. By W. K. Brooks of Baltimore, Md.

DESCRIPTION OF A NEW GENUS AND SPECIES OF ALCYONOID POLYPS FROM JAPANESE WATERS, WITH REMARKS ON THE HABITS AND STRUCTURE OF RELATED FORMS. By Robert E. C. Stearns of San Francisco, Cal.

ON THE POLYMORPHISM OF LYCAENA PSEUDARGIOLUS. By W. H. Edwards of Coalburg, W. Va.

ACHAENODON FROM THE BRIDGER EOCENE BEDS. By Henry F. Osborn of Garrison-on-Hudson, N. Y.

AN EXAMINATION OF SOME CONTROVERTED POINTS OF THE PHYSIOLOGY OF VOICE; ESPECIALLY THE REGISTERS OF THE SINGING VOICE AND THE FALSETTO. By T. Wesley Mills of Montreal, Canada.

BLASTESIS TRIDENS; A PEAR TREE FUNGUS. By William H. Seaman of Washington, D. C.

PROGRESSIVE GROWTH OF THE DERMOID COAT OF THE MEMBRANA TYMPANI. By Clarence J. Blake of Boston, Mass.

REMARKS ON THE TURBELLARIA. By Wyllis A. Silliman of Clarkson, N. Y.

NOTES ON THE FLORA OF THE ROCKY MOUNTAINS OF IDAHO AND MONTANA. By Sereno Watson of Cambridge, Mass.

ON THE VENOMOUS PROPERTIES OF THE TOAD. By J. A. Crevier of Montreal, Canada.

NOTES ON SPIDERS. By Henry C. McCook of Philadelphia.

SECTION G.

HISTOLOGY AND MICROSCOPY.

ADDRESS

BY

PROFESSOR ALBERT H. TUTTLE,

VICE PRESIDENT, SECTION G.

FELLOW-MEMBERS OF THE SECTION OF HISTOLOGY AND MICROSCOPY:—

IT is my esteemed and honorable privilege to preside over the first session of this body held with its present organization and standing as an independent Section of the Association.

In assuming the duties and the honors conferred upon me, I find my mind reverting to the antecedents of this Section and the days of its beginnings. Some, at least, among you will recall with me the daily gathering of a little company in an out-of-the-way room during the Salem meeting of the American Association in 1869, to which the name of "the Subsection of Microscopy" was first given.

Long shall I remember the sessions of that meeting, half formal, half informal. There we debated (or listened while others debated) gravely whether or no the "nineteenth band" had really been resolved. We triumphed unitedly in the clearness of the demonstration by one of the more expert of our number of the "basket-work" upon *Surirella gemma*,—*ultima thule*, then, to us of test-objects; and some of us, at least, looked for the first time, at and through what was for us the crowning wonder of the day—a Wenham binocular! We discussed there in simple faith the merits and demerits of "that quality in an objective termed penetration," without a shadow of doubt as to whether we agreed as

to what we meant by the expression, or any dream of the latter-day skepticism as to the very existence of the "quality" we so earnestly discussed. We listened eagerly while the late Mr. Bicknell described the method by which he made his then unrivalled injections, the open secret of which appeared to be, when all was told, that the way to make good injections (or good anything else) was to take each step intelligently, carefully and correctly; and we gazed with interest, if not with admiration, at a demonstration, which some who were present then will perhaps recall, of how a microscope can be made to serve as almost anything that it was not intended to be, if you only modify it sufficiently, and add pieces enough.

This sounds like childish talk, but such was the infancy of the body that sits here to-day; and this, let me add, was in what might almost be called the childhood of microscopy in America. True, there were already in this country many excellent microscopes (including a due proportion of the wonderful binoculars) and a goodly number of excellent microscopists, not a few of whom had risen far beyond the level indicated by the scenes that I have described. True, much excellent research, some of which stands to-day as no less excellent, had been carried on in America long before the times of which I speak, by solitary workers, scattered here and there over the eastern states, the names of many of whom will be recalled by each of you as I speak; but that general interest in the microscope and its revelations which forms so large a part of the intellectual activity of to-day was as yet hardly known, and among those devoted to scientific and even to biological research the microscope was far too often an infrequent and unimportant auxiliary.

If microscopes were not as numerous then as now, neither were they as easily to be had. In that day he who wanted to buy a microscope had to hunt for it. To-day I had almost said that a man must be rather ingenious to avoid buying half a dozen. Then, he who wished an instrument from a foreign workshop had to make it the subject of a special order; to-day half a dozen importers carry extensive stocks of the instruments most likely to be in demand. Then, as regards American-made microscopes, if a stand was wanted, the buyer looked (and not unwisely) first of all, to a little shop in Philadelphia where labored and still labors—long may he do so—one, the thoroughness and fidelity of whose handi-

work alone enable him still to hold his own against a score of competitors, who by hand and by steam are putting yearly upon the market countless instruments of every conceivable plan and of all grades of excellence. If one sought for objectives of the highest character he must perforce seek the atelier in Boston of one who then stood without a rival on the American continent. Though his work is, as I believe, yet to be surpassed, one may look to-day not unprofitably in three or four other directions to find makers of thoroughly excellent objectives; objectives, if you choose, whose available angle of aperture includes, as a still skeptical friend of mine dryly puts it, by far the greater portion of a circumference: while as regards low powers and moderate angles, the market now abounds with excellent American-made objectives.

I would not overlook the fact that there were other makers in this country at that time; men who made excellent stands, and men who made excellent objectives; but I am confident that I do no injustice to others in saying that the two whom I have indicated were the only ones at all widely known.

The increase I have indicated in the number of prominent makers, is, of course, directly associated with an increase alike in demand and in production; but who can realize the magnitude of that increase? The makers of that day counted their results by dozens; the yearly aggregate production of to-day must be told by thousands; and it can then hardly keep pace with the demand, in spite of the large additional importation of English, French and German instruments. Competition has, as usual, been followed by the two substantial benefits of improvement in quality and reduction in price. I think sometimes almost with regret of my first venture, recalling how little I then obtained for my scant eighty dollars, and seeing how much that is really excellent, both mechanically and optically, can be commanded by the beginner of to-day for the same or even a smaller sum. At that time, too, books, appliances of every sort, and, most valuable of all, personal hints and suggestions from those of experience, were scarce and hard to be obtained. To-day few communities are so small or so remote as not to furnish all three in varying quality.

Such, as I recall them, were the impressions made upon a young western teacher, himself then a beginner (and even now but little more), of the times and the sessions of the first Sub-

section of Microscopy of the American Association. The effort made at that time to bring together the workers with our favorite instrument was renewed from year to year at succeeding meetings, the movement gathering strength at Troy, at Indianapolis, at Dubuque, at Hartford, at Detroit, at Buffalo (where the Subsection acquired the dignity of a permanent organization), and at each succeeding meeting, until the result has been obtained which we witness to-day.

While the vastly increased interest manifested in America to-day in the microscope and its use must, of course, be chiefly attributed to that phenomenal movement in biological science which we have been so happy as to witness, and from which we have derived so many and so varied benefits, I think I may justly claim that no small credit should be given to the yearly meetings of the Subsection, with their usual exhibitions and soirees, giving to the people of the various communities in which we have met notice at least of the existence of such a thing as a microscope, with some scant glimpses of the wonders that it has to show; awakening here and there an interest that has ended in something more than mere curiosity-seeking and amusement; drawing together and uniting those of a common interest: to which last effect may, I think, be at least indirectly traced a result whose future offers great promise of good, the organization of the young and vigorous American Society of Microscopists; an organization in whose power it is to do a great amount of excellent work that could be done by no other means as well. The influence exerted in one way or another by our half score of sessions would, I think, if it could all be measured, far exceed in value the papers that have been read at them, though these have been neither few nor unimportant.

Such has been our prentice-work; we stand henceforth among our equals as a Section. It would be pleasant here to pass from recollection to prediction, and to spend the remainder of the half-hour in the discussion of great possibilities; we shall do otherwise, however, if we give heed to the pithy suggestion contained in an ancient but excellent volume of practical philosophy (with which I trust that most of you are familiar) concerning boasting and the putting on of armor.

And this the more wisely because another topic of a far different nature presents itself, and, I may almost say, demands attention.

Before telling what we intend to do, we are in a certain way called upon to defend our right to be. It is commonly known, I think, that not only in the Standing Committee of last year, but also throughout the membership of the Association, there have been felt and expressed grave doubts not only of the necessity but also of the propriety of raising the Permanent Subsection of Microscopy to any higher dignity: while the extension and associated limitation of its province which the new name of the Section connotes, though they have so far satisfied these objections as to lead to its establishment, have called forth from others even stronger criticism, as raising to the rank of a distinct department of science that which they had hitherto regarded as holding a far subordinate position. Respect for the candor and the ability of our critics, as well as for their zeal for the wise ordering of the affairs of the Association, would of itself demand that these objections be met and answered. I shall deal with them to the best of my ability; should my arguments fail to carry conviction, I trust that the weakness will be attributed to the advocate rather than to the cause.

To the question first referred to, it has been considered a sufficient answer to urge the fact, which will, I think, be universally conceded, that there is no other instrument of research known to scientific men that has attained so high a degree of development as the microscope; no other whose parts both mechanical and optical have been the subject of so much prolonged, earnest, and fruitful discussion; none which has so many and so varied accessory appliances pertaining to it, in relation with its almost endless diversity of uses. Furthermore, this high degree of development calls for a proportional degree of training in the worker. There is a great amount of study of the instrument itself demanded of him, side by side with an equally great amount of labor that may be regarded as gymnastic in its nature (in which we see the final cause of diatoms for all but cryptogamic botanists, and of ruled plates for all mankind); while the various processes by means of which objects of study themselves are brought into a suitable condition for examination and preservation call for no little skill and practice. It is urged, therefore, in view of the peculiar character and wide applicability of the instrument in question, and of the extent and arduousness of the discipline that its skillful use requires, that they who have in greater or less measure acquired this

discipline are by virtue of that fact drawn together, forming a body of sufficient coherency to be entitled to independent standing as a Section of this Association.

I have endeavored to state the argument in favor of a Section of Microscopy briefly but fairly. Love of justice makes me anxious not to understate it, for, as many of you are well aware, I am one of those who have questioned strongly the propriety and wisdom of organizing such a Section; and I must confess that after an honest endeavor to put the case as strongly as possible in my own mind for those who think otherwise, and to convince myself of the truth, whatever it may be, I am still of the same opinion.

For I do not see what more we can say of Microscopy than that it is a technique; so elaborate, so complex, that its acquirement in any considerable measure is in itself a valuable discipline, but a technique still. No one feels more strongly than I its fascination; no one is more alive to the feeling of sympathy that unites the brotherhood of the tube. I have as lively and practical faith in the value of societies of microscopists and in journals of microscopy, as I have in other technical societies and periodicals. I believe, as I have already said, that they can under certain circumstances do a work for science that can be done in no other way as well; but I cannot see the propriety, in an Association whose aim is first, last, and always the advancement of science, of establishing a Section on a purely technical basis.

Nor, were its propriety conceded, would it, in my judgment, be either wise or expedient to do so, considering either the efficiency or the stability of such a body. The almost universal applicability of the microscope is in itself a bar to that unity of purpose which should be the first characteristic of a working organization. I can perhaps illustrate this to advantage by referring to another instrument of research which has attained in its various forms and uses a very high degree of development, and whose use calls for no small amount of training and experience: I refer to the polariscope. Were we to gather into one body all the chemists, physicists, mineralogists, and biologists who make more or less extensive use of this instrument in scientific investigation (to say nothing of the various groups of those who apply it extensively to the affairs of every-day life) we might make a Section of Polariscopists; but it would soon unmake itself.

I know that this will be regarded by some as a false analogy; and that appeal will be made to the prevalent idea — far too prevalent in my judgment, since it is largely responsible for that diffusion of energy whose practical exponent is the fact that the number of microscopes sold yearly in this country is in proportion to the number of valuable original contributions on microscopic subjects annually produced as over fifty to one — that the expert microscopist is he who, having a good stand and a number of good objectives which he can handle dexterously, has also a large cabinet of objects, mostly of his own preparation, illustrating as many of the various applications of his instrument as possible; one who, not specially a botanist, is at home with diatoms and algæ; not calling himself a zoölogist, has a certain familiarity with infusoria, entomostraca and rotifera; not claiming to be an histologist, has cut and stained numerous sections of both plants and animals; neither a geologist nor a mineralogist, has ground, polished and mounted many sections of rocks and corals; not a chemist,— but why extend the category? one, in fine, who, in accordance with a tradition whose history would be full of interest had we but time to trace it, has acquired skill enough and expended brains and labor enough in a dozen fields to have made himself a master workman in any one of them. I confess that I cannot but regard this idea as an idol of the tribe: to my mind the skillful microscopist is he whose hand has learned all necessary manipulations, whose eye has been trained to nice and quick distinctions, and whose judgment, resting on accumulated experiences, can interpret rightly the appearances presented to his eye: and he is most successful who having chosen a definite field of labor has worked most diligently therein to increase his own or the world's stock of knowledge.

I would not be unmindful of the fact that there are those who, working with the microscope, have entered many fields, and gleaned, nay, reaped richly in them all; but they are few: neither would I pass unkindly criticism on those who using the microscope avowedly only as a recreation find pleasure and substantial profit in passing from time to time to fresh fields and pastures new; I take it, however, that the Sections of this Association are organized primarily, if not solely, with the view of bringing together most closely those whose labors have most in common; and however it may be with objectives, we may be sure that for Sections of this

Association not only penetration but definition also is in inverse ratio to angular aperture.

It was therefore the judgment of the Standing Committee (and this judgment was ratified by the vote of the Association at large) that this Section should be established not purely nor indeed chiefly for that technique which the word Microscopy implies; neither should that name stand as the label for a receptacle for communications having only this in common that the objects of investigation are too minute or of too fine a structure to be examined by the naked eye. They have, instead, established a Section devoted to that department of science whose investigations must be wholly carried on by the aid of the microscope, and to the technique of the instrument as auxiliary to it. As in the Section of Chemistry papers discussing appliances and methods whereby a more accurate analysis or a more complex synthesis may be obtained find place and welcome, or in the Section of Physics discussions of means as well as of results of investigation are gladly received, so here the presentation and description of new apparatus, the discussion of new methods of preparation, of manipulation, or of demonstration find appropriate and ready audience; but I present as my opinion, which I hope will find echo in your judgment, that papers on subjects pertaining to micro-chemistry (if there be such a thing as micro-chemistry) will best be read to chemists: that papers in what are termed micro-geology and micro-petrography will find readiest and most appreciative listeners in the Section of Geology. I would even suggest that a paper on the entomostraca or the rotifera, particularly if it deals with the morphology or the systematic position of the form or group in question, will find its normal auditors among the zoölogists in the Section of Biology.

What then remains to us? I have spoken of that department of science whose investigations must in the nature of things be carried on wholly by the microscope, as being designated by the organic name of the Section as its particular field of labor. I say, that department of science; knowing that the expression must call forth that adverse criticism to which I have alluded and to which I must now address myself. It has been urged that Histology is in substance but a finer Anatomy; a dissection made with the eye where the scalpel cannot go; at best no better than the handmaid

of Physiology: and that to dignify it with the rank of a distinct department of science is to give to the servant the honors due to the mistress.

This, I acknowledge, is the traditional view: and this, unfortunately, describes only too well the Histology that is too often studied and too often taught to-day: but I am persuaded that the beginnings have already been made of a newer and a greater Histology than this: the leaven that has leavened the whole lump of biology in the last quarter century is working here as elsewhere; and the resulting recombination of ideas has given us a conception of the significance of the phenomena of cell-life of which a previous generation hardly dreamed.

As a rich and fertile soil whose yearly harvests pay abundant rental for its tillage may long overlay unsuspected mineral wealth of far greater value, so it often happens that a discovery that proves itself at once of great practical importance may remain for a long time with its philosophical significance unrecognized. The results of that enquiry into the structure of the organs of living beings which the immortal discoveries and inventions of Lister and his contemporaries first made possible were found to have so much practical value to the physiologist that they were at once appropriated to his use. This analysis did not at first go beyond an idea of fibres, cells and membranes; it did not seek and only vaguely dreamed of any kinship between them; later, when their essential affinity was at first surmised and then demonstrated, the fact was, save by a few, regarded as of secondary importance to what had now come to be commonly known as Physiological Anatomy.

To-day, however, the situation is changing rapidly. The conception long since advanced by one of the greatest of living thinkers that the continuous processes of adjustment of relation which make up all we know of life are the resultant or algebraic sum of the activities of the structural elements which make up the living being, and that organs and organism alike must be regarded as the consequence of the integration of countless cells or nucleated corpuscles, is yearly gaining wider acceptance. Some one has said that the doctrine of the survival of the fittest is the equator of the sphere of biology: may I express the opinion that the conception to which I have referred will one day be for physiologists at least its prime meridian? However this may be, this conception,

it will be readily admitted, has greatly enhanced the significance and the dignity of the study of cell-life.

There is another and even more important relation of Histology to Biology that is very apt to be overlooked by those that regard it as adequately described by the name of Physiological Anatomy. I refer to its relation to morphology, and its bearing on questions of affinity and classification. Were it proposed to establish in this Association a Section for Comparative Anatomy, or for Comparative Embryology, no one, I am sure, would for a moment question the propriety of such action in either case: I think I am warranted in the assertion that Comparative Histology stands to-day where the former did at the beginning and the latter at the middle of the present century, and in the opinion that the day is not far distant when its indications will be as confidently received and as diligently heeded as those of either of its allies. From a number of illustrations that occur to me I will take time to mention but two. You are all, doubtless, familiar with the homologies that were once regarded as established between the Trematoda and the Cestoda: you are equally well aware of the doubts that have been thrown upon them in later days by the failure to discover associated embryological resemblances: in my judgment these doubts will disappear in the mind of any one who will carefully study the histology of a tape-worm and of a fluke. As a representative of morphological problems requiring to the full as much aid from Histology as from Anatomy or Embryology, I would suggest the great one yet unsolved presented by the nervous axis of vertebrates; I am convinced that we shall wait for a clear and accurate conception of the morphology of the brain, and of the homologies existing between the various parts (our present ignorance of which we have buried beneath a mountain of terminology) of the widely differing brains of mammals, sauropsidians, amphibians, selachians, teliosts and marsipobranchs, until, beginning at the bottom of the series and ascending along diverging lines, we have worked out by histological methods the structure, development, position and connections, both primary and adapted, of every mass of gray matter in that galaxy of puzzles.

The claim of Histology to rank as a distinct department of science might well be allowed to rest upon the amount and the nature of its increased importance, both physiological and morpho-

logical, that I have indicated: there is another consideration in its favor, however, that I regard as far weightier. I have hitherto spoken of tissues and their activities as related to the functions and the structure of organs and organisms. I would now call your attention to the value of the study of cell-life considered in itself. We have most of us had our eyes opened for some time to the magnitude and the significance of the struggle for existence that goes on at all times between organisms; it is only recently, however, that our attention has been definitely called to the struggle of parts within the organism, and to the probable truth that every organ is where it is and what it is through a long series of adaptations to its environment. And this analysis must be carried still farther: for as the body is built up of its organs, so, as I have already said, is each organ the result of the integration of its cellular elements. The great law of biology that gives order and system elsewhere leaves no chaos here; and our Histology is not complete until every tissue and every cell has been studied in its light.

Here, then, is our province. Is it not ample enough? It embraces the study of cell-life, in all its bearings, in plant and animal alike. Since sound fundamental ideas of the laws which govern cell-life can only be obtained by prolonged and careful study of those plants and animals which consist but of single cells, or of undifferentiated social aggregates of cells, I claim for hearing here all papers relating to the Protozoa and the Protophyta, including particularly the ferment-organisms on account of their frequent pathogenetic function. Before us come, of course, all papers dealing with cell-life in the higher organisms; papers alike upon the morphology of cells and on higher morphological questions treated by histological method; papers alike on the development of cells and on the structure and significance of embryonic layers and tissues. The newer Histology is still the helpmeet if not the servant of Physiology; while large place will be given here to questions of cellular teratology—for what is Pathological Histology other than this? Lastly, but not least by, we shall welcome here all papers and discussions calculated to help us either to better microscopes, or to a better use of those that we now have.

I have felt, for special reasons, that it was particularly incumbent upon me before presiding over the first session of the Section of Histology and Microscopy, to present to you my interpre-

tation of the organization of the Section, and my idea of its future policy. Since this meeting has it in its power to do much to determine the future line of action of the Section, I trust that we shall all agree upon the one here presented or a better. Should your judgment approve of the view that I have taken, I am confident that you will join me in a modest expression of the desire that as in the past the Subsection of Microscopy did much to cultivate a widespread popular interest in the microscope, so in the future the influence of this Section may do much to concentrate and economize the energies of microscopists, and of the hope that as the parent Association represents the aggregate of American scientific research, so our little body may one day come to be the representative of all who labor with us in our chosen field.

PAPERS READ.

PLANT-"CELLS" AND LIVING MATTER.[1] By LOUIS ELSBERG, of New York, N. Y.

[ABSTRACT.]

THIS paper, illustrated by carefully prepared drawings, records the discovery of perforations in the cellulose walls of plant-cells giving passage to protoplasmic offshoots from the interior and demonstrates how in a plant, as in an animal, the living matter throughout is uninterruptedly connected.

THE THIRD CORPUSCULAR ELEMENT IN THE BLOOD. By W. OSLER, of Montreal, Canada.

[ABSTRACT.]

BY the third corpuscular element in the blood I mean certain small colorless discoid bodies $\frac{1}{3}-\frac{1}{6}$ the size of red blood corpuscle, quite common in the blood of mammals and about which much discussion is at present taking place. They circulate free in the blood with the other corpuscles, but in the withdrawn fluid tend to adhere together forming irregular clumps or collections. They correspond to Zimmerman's "elementary corpuscles," to the individual elements of Schultze's granule masses, to the Hæmatoblasts of Hayem and to the "Blutplatchen" of Bizzozero. Scanty in the blood of healthy adults, they abound in the blood of the young and in that of individuals in an anæmic or cachectic condition. The most suitable object for their study is the blood of the young rat, in the vessels of which they can be seen as isolated corpuscles.

[1] Published in the Quarterly Journal of Microscopical Science, London, January, 1883, p. 87.

The paper considered, first, the possible connection which these bodies have with the development of the blood; and second, their influence in determining the formation of fibrin.

The Development of Blood Corpuscles in the Bone-marrow. By W. Osler, of Montreal, Canada.

[ABSTRACT.]

A brief resumé was given of the views current regarding the development of blood corpuscles.

The red marrow always contains a form of corpuscle, thought at one time to be peculiar in the mammal to the blood of the embryo, viz., the nucleated red cell. This is an intermediate stage between certain of the colorless marrow cells and the ordinary red corpuscles. The stages as traced by the author are (1) the colorless marrow cell becomes less granular, the protoplasm gets more homogeneous, (2), it becomes gradually tinged with hæmoglobin, the tint becoming progressively deeper until it may attain that of the ordinary corpuscle. The nucleus remains unaffected. In this form they constitute the well known nucleated red corpuscles. (3) The nucleus or nuclei gradually disappear, often first breaking up into smaller portions and the entire protoplasm of the cells is uniform and undifferentiated.

These observations of the mode of production of the red non-nucleated cell in the marrow accord in the main with those of Neumann. The view expressed by some authors (Rindfleisch *et al.*) that the nucleus migrates from the cell is believed to be incorrect. The appearances on which it is based are known to the author, but they are believed to be the result of physical changes.

Note on the Microcytes in the Blood and their Probable Origin. By W. Osler, of Montreal, Canada.

[ABSTRACT.]

Microcytes are exceedingly tiny red blood corpuscles $\frac{1}{3000}$ to $\frac{1}{5000}$ of an inch in diameter which are met with (1) in the blood

of embryos and of the new-born. (2) Occasionally, and in small numbers, in the blood of healthy individuals. (3) In certain diseases, particularly the severer forms of anæmia and (4) after severe hemorrhages.

No satisfactory explanation has been given of their origin. The author has, in the spleen tissue and in the bone marrow, seen their production by a sort of budding from the ordinary red blood corpuscles. In the conditions above mentioned, particularly after hemorrhages and in profound anæmia the red corpuscles often present great irregularities in outline and projections (Porkilocytosis of Quincke). In fresh specimens of spleen and bone marrow the author has seen these little projections become detached from blood cells — moving about in the currents in the slide — and when so detached they presented all the characters and appearances of the microcytes in the same field.

The Histology of Uterine Fibroid Tumors. Illustrated by a Series of Micro-photographs.

By Henry O. Marcy, of Boston, Mass.

[ABSTRACT.]

The histological elements of the uterine fibroid tumor have usually been considered to consist either in very large share of connective tissue, or of a blending of connective tissue with muscular fibres; *i. e.*, a fibrous or a fibromyomatous growth. The objects of the investigations, of which the micro-photographs are shown in illustration, have been to determine more thoroughly not only the composition of these growths, but also their relations to the surrounding uterine tissue. It is scarcely pertinent to the objects of this meeting to enter into a detailed discussion of the pathological changes which ensue, but the subject is rather introduced to show the advantages of micro-photography and its easy reproduction by the heliotype and other similar processes. Its great value in histological and pathological teaching is undoubted.

The sections were made by the use of Dr. Hailes' freezing microtome and in their preparation and mounting, as well as photo-

graphing, I am largely indebted to the valuable assistance of Dr. Alfred F. Holt, of Cambridge.

The photographic work was done in the studio of Mr. Black, of Boston, an expert in this direction well known to America, and in my judgment challenges favorable comparison with even the justly celebrated work of Dr. J. J. Woodward of Washington.

The conclusions reached, as the results of these observations, are briefly: that the origin of these growths is not in reproduction of connective tissue cells, having as a point of departure a vessel or a nerve, but that it consists of an extraordinary development of true muscular fibres of the uterus, usually arising from several centres, resulting in a multi-lobular growth, and may consist of one or more independent tumors.

In their development, these fibres are more or less altered in character by compression, and when of considerable size, by lack of nutrition. The connective tissue stroma is not usually increased, but is often less than that of normal uterine tissue. The tumors are rarely supplied with vessels of any considerable size but depend for their nutrition in large share upon the blood supply of the network of surrounding vessels which is usually exceptionally developed and the vessels are often ectasic in character.

The capsule is composed of the muscular fibres of the surrounding uterine wall, its cells greatly altered by continuous tension and compression, and is not in any degree a proliferation of connective tissue.

Around the tumor there are usually several of these capsular layers which may be peeled or stripped off. They are so changed in their character that it is not remarkable they have been considered as made up from connective tissue.

On the contrary the tumor attachment to these layers, and to the more dense uterine structure, is very slight and is composed of delicate bands of connective tissue. The histogenesis of the myomatous tumor and especially of its relations to the unchanged surrounding uterine structures, in its surgical bearing, is of the first importance.

Some Vegetable Poisons. By J. T. Burrill, of Champaign, Ill.

[ABSTRACT.]

During several years I have occasionally noticed within the closed cells of apparently healthy plants of divers kinds, numerous actively moving particles which in form and motion were indistinguishable from some kinds of lower organisms we call bacteria.

Knowing that such cells have for their exterior a cellulose membrane in which there are absolutely no imperforations large enough to be discovered by the best and highest powers of our modern microscopes, the idea of living organisms gaining entrance to the cell and dwelling imprisoned there was further stifled under a presumed impossibility. This also must have been the case with many other botanists and microscopists, for the phenomenon is by no means rare, yet unrecorded so far as I know.

But the evidence is too strong to admit of further doubt that bacteria of several kinds do inhabit the cells of apparently healthy plants.

Some of the best instances observed by myself have been in the cells of certain mould fungi and especially in the fertile threads of *Ascophora mucedo*, a common black mould on damp bread, decaying fruits, etc. In the higher plants the most satisfactory cases have been in the cells of hairs through whose transparent walls everything within can be easily seen; but sections of tissues frequently give evidence of the same thing in the deeper layers.

These things, however, did not attract any considerable attention until it became manifest to me through a series of investigations, that bacteria cause *disease* in plants, especially such as we call "blight" in the pear tree, apple tree, etc. The older observations were then recalled, and upon further study, satisfactory evidence of the existence in plant cells of veritable living organisms, capable of propagation outside of the tissues was gained.

Surprising as these results are, they may not be more so than the known fact that bacteria occur in and on the bodies of healthy animals. In human beings they are found in the scurfy skin of the scalp, in the perspiration from the face, and especially in the epidermal exudations from portions of the body covered with clothing, in the mouth, the stomach and the intestinal tube, and

sometimes in the blood. Some of these may perform important physiological functions and therefore may be essential to our well-being, while it is positively known that some specific kinds are our mortal enemies, constituting the active principle of pestilence and contagion.

Another circumstance called still further attention to the living organisms in plants. A lady was severely poisoned with *Rhus toxicodendron*. The exuding serum from the inflamed part, and the leucocytes from the miliary vesicles, were found to contain swarms of bacteria of the genus *Micrococci* as defined by Cohn. It was demonstrated that these organisms transferred from the inflamed to healthy parts of the body communicated the severe malady, that they rapidly increased in numbers and were a true *contagium vivum*.

Search was now made and similar organisms found in washings of fresh leaves and in the juices of the poisonous plant. To rigidly test the matter, a little of the milk sap was secured free from anything not contained in itself and diluted with distilled water to such an extent that the fluid had a slight milky appearance, yet was nearly transparent. A minute drop of this was placed on my own arm after marking the spot by clipping the hairs thereon. The strongest chemical caustic known would not thus cause any serious lesion of the skin, diluted as it was a hundred fold. No sensation was produced at the time, neither was there any marked indication of effect during ten hours following. But on awaking the next morning, eighteen hours after the application, the spot was evident from its redness, and there was a slight sensation of heat in the part. These symptoms rapidly increased, becoming strongly marked by the twenty-fourth hour, and at the forty-eighth my arm was much inflamed and considerably swollen. There was intense itching; the inflamed area increased until by the sixth day my whole arm was involved. After about ten days the malady had evidently run its course and gradually abated. The microscope showed the characteristic bacteria; these were again proved to transmit the trouble to a healthy part of the skin and there increase as before. Indeed nearly my whole body became more or less affected and the difficulty became chronic in certain places, lasting for months though less severe than the original inflammation.

There is positive proof that individuals are sometimes poisoned

by simply approaching, without touching, this plant as well as some of its congeners. Here again, the only explanation is that the minute quantity of the poison received is capable of increase by growth. Having seen by the aid of the microscope a something from the plant, and also in the inflamed tissues, which does thus increase and which being placed on a healthy part reproduces itself and the disease, we conclude this something is the *vera causa* of the infection.

When the living organisms have gained access to the deeper layers of the skin, it is difficult to reach them by medicinal applications; but in other cases, any antiseptic substance tried proved quite efficacious. If a little diluted carbolic acid is applied to the skin soon after the poisonous juice has been experimented with, no evil consequences follow. Quinine seems to be an excellent remedy.

The organism is spherical, usually single, but often seen in twos, seldom in strings of four or more, about $\frac{1}{2}\ \mu$ (.00002 inch) in diameter.

The above, though the best and clearest, case of plant poisons being due to bacteria is not the only one. There can scarcely be a doubt but that the ill effects of numerous species of different orders and classes are produced in the same way. I will mention only the poisonous fungi sometimes unwittingly used for food.

There are two pretty well marked modes of mischievous action from the use of such plants, operating either singly or combined. In one case the bad results are soon made known by pain and vomiting, in the other the unfortunate victim perceives nothing wrong for six, ten or more hours after eating the poisonous food. The first has been ascribed to a peculiar acrid, the second to a narcotic principle in the mushrooms. The former, though violent, is not nearly so dangerous as the slower action of the second. When both act together, or rather one follows the other, peril is imminent.

The first symptoms of the latter kind are usually a heaviness of the head, a confusion of ideas, an inclination to stupor; these are succeeded by sickness at the stomach, vomiting and purging and a burning thirst. Convulsions follow, cold perspiration appears and death occurs either in the agonies of pain or in a profound and hopeless stupor.

Four persons died in this way in Brazil, Indiana, in October, 1881, from fungi cooked and eaten at noontime, Oct. 16th. Two died in the afternoon of the eighteenth and two on the twenty-second. The last, a strong man, was able to walk about the room until a few hours before his death. Towards the last there was no particular pain; but decomposition evidently set in before death.

I relate these terrible accounts to show what effects may follow the use of such poisonous plants for food, yet I have myself eaten in quantity, and have known many others to eat, of the very same species which caused these deaths in Indiana. The plant is a very large parasol-like fungus sometimes found in great abundance in pastures and meadows (*Lepiota Morganii* Peck). In the making of catsup it is well known that all sorts of mushrooms are collected and used for the purpose without ill effects. I was once asked to taste of some supposed mushrooms which had been put into strong vinegar some weeks before and which, thus pickled, had been eaten with nothing but pleasant remembrances of the fine flavor; yet these were certainly poisonous species. The vinegar had also been swallowed without injury, showing that the poison was not simply extracted; was it not killed? Is not the immunity mentioned above explained better by the cooking than by anything else? The fungus just named belongs to a very suspicious group and it was only by careful experiment that I came to eat it and then only after thorough cooking. To this it is usual to answer that the poison is volatile and is driven off by heat; but why so tardy in its action followed at length by such fearful results? That living micro-organisms exist in such fungi is certain; do they not in some instances cause the mischievous results? The bad effects occurring soon after swallowing the material is quite surely due to a poisonous substance and it is still doubtful what effect cooking may have upon this; but the consequences are never so severe as with the slower acting so-called narcotic effect.

Upon a careful perusal of the exaggerated injuries ascribed to the famous Upas tree there does not seem to me any other theoretical interpretation of the facts so suggestive as this of the action of living organisms tenanting its tissues.

NOTES ON SOME OF THE STRUCTURAL AND PHYSIOLOGICAL PECULIARITIES INCIDENT TO THE DISEASES OF FRUITS.

By D. P. PENHALLOW, of Mountainville, N. Y.

THE facts contained in the following notes have been obtained as the joint results of investigations conducted on the one hand by Dr. C. A. Goessman, of Amherst, Mass., who has examined the subject from a strictly chemical standpoint; on the other hand by myself, using the microscope and viewing the question from a botanical standpoint. My object in presenting these notes to the Association is not to bring forward any well established conclusions, but rather to offer a report of progress upon work which demands much time and critical examination before it will be safe to present anything more definite.

Our work has been largely confined to studying the peculiarities of peach trees affected with the yellows, and the following remarks will bear directly upon that subject alone.

In his studies upon this subject, Dr. Goessmann has shown from ash analyses, the relative importance of certain mineral constituents in the vegetable economy and the relation which they bear to a healthy and diseased condition[1]. The following analyses, calculated upon a basis of 100 for the five constituents given, will show the importance of the relations first referred to, comparing the fruit and branches of the healthy, with those of the diseased tree.

Fruit of Early Crawford.

	Healthy. Per cent.	Diseased. Per cent.
Ferric oxide	0.58	0.47
Calcium oxide	2.64	4.68
Magnesium oxide	6.29	5.48
Phosphoric acid	16.03	18.07
Potassium oxide	74.46	71.30
	100.00	100.00

Branches of Early Crawford.

	Healthy. (Restored)	Diseased.
Ferric oxide	0.52	1.44
Calcium oxide	54.52	64.23
Magnesium oxide	7.58	10.28
Phosphoric acid	11.37	8.37
Potassium oxide	26.01	15.68
	100.00	100.00

[1] Observations Regarding the Yellows of the Peach, C. A. Goessmann, Ph. D. Trans. soc. for Prom. of Agricultural Science, 1882, Montreal.

From these analyses, the relative importance of calcium and potasssium oxide is shown in a striking manner; a decrease of the former in healthy structure corresponding very closely with an increase in the latter.

Our studies conducted with the aid of the microscope have served to throw much light upon this subject, and not only have they confirmed the deductions from a chemical standpoint, but they appear to be in accord with the earlier investigations of Nobbe and Schroeder on rye and buckwheat, during which they demonstrated the superior value of chloride of potassium over the sulphate, as establishing in the plant a normal distribution of assimilated material to the growing parts.

Growing upon diseased plants, and often producing by their action, some of the more conspicuous features of the disease, are fungi of various forms. These, however, are such as may be generally found upon decaying organic matter, such as old stumps, and do not belong to that class of fungi generally considered capable of immediately producing disorganization in highly vitalized structures. Moreover, while found upon the outer dead bark, and, under favorable conditions in the interior structure, the same forms occur on both the diseased and healthy trees and what is more striking, they also are to be found upon trees which once badly diseased are now restored to a condition of vigorous health. We are led to believe, therefore, that while these parasitic growths do produce certain well defined effects under certain conditions of the plant, their relation to disease is of secondary importance; we must seek the cause of disorder in conditions of soil and food.

An examination of the cellular structure and cell contents has shown:—

1. Healthy wood shows comparatively little stored starch in the bark.

2. Diseased wood shows an abnormally small development of the cells and the invariable presence of large quantities of starch in the bark and pith.

3. There is little or no difference between the tissues and cell contents before and after the leaves fall.

These conclusions appeared justifiable at a comparatively early stage of our inquiry and have been abundantly confirmed by subsequent examinations. A general predominance of and correspondence between calcium oxalate and stored starch in diseased

structures, seems to offer another confirmation of the chemical deductions previously referred to.

It has been customary, until a very recent date, for students to approach the subject of diseases of plants from a strictly botanical standpoint, giving little or no weight to other considerations. The old view, stoutly maintained by some even at the present time, that these diseases originate in fungoid growths, gave rise to the application of certain remedial measures, the aim of which was to arrest disease by destroying the parasite. No successful case of such treatment has yet been recorded.

The new method while recognizing the important connection of parasites with disease, aims first to promote a highly vigorous condition in the plant and thus enable it to throw off the unwelcome guest. Badly diseased trees, treated upon this principle, have been restored to vigorous health and for the past two or three years have borne fruit commanding the best market prices.

From this brief statement and review of the case, it will be apparent that in the future, we must not depend upon either the microscope or the chemist alone, but if we are to obtain a correct solution of these difficult and most important questions, we must unite by rational method the deductions of the chemist with those drawn from microscopic study.

A NEW FORM OF CONSTANT PRESSURE INJECTION APPARATUS.

By WILLIAM LIBBEY, JR., of Princeton, N. J.

IN studying the circulatory system, the means of injection are an indispensable aid. This operation can be performed in several different ways, but the most satisfactory method would of course be that which would in the simplest manner utilize the materials used and at the same time place them under the most perfect control of the operator.

In the piece of apparatus described below, the wants of my laboratory have been especially consulted, and the object kept in view was the construction of a machine which would perform the most delicate injections in a satisfactory manner. In a sense, it may be said that this object has been realized; but, as the element

of good judgment can never be made part of a machine, the degree of success will still be in proportion to the skill in manipulation. All such machines must have a certain similarity in their construction, but may differ essentially in the arrangement of their several parts, and for this reason no great originality is claimed for the apparatus; the method of the operation in the machine under consideration is as follows:—

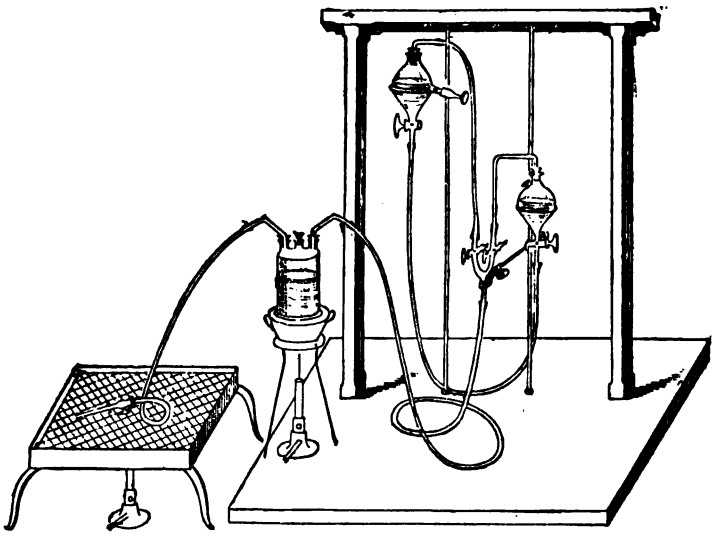
We will suppose the animal or organ to be injected to have been prepared as usual and placed over a water bath arranged to secure a proper degree of temperature during the operation; and, the injection mass having been also previously prepared and placed in a three-mouthed wolffs-bottle (prepared as a wash-bottle with closely fitting rubber corks) which should, of course, be placed over a water-bath to keep the mass liquid.

The source of the power utilized is twenty pounds of mercury which, by changing position from a higher to a lower level, forces the air out of one of two glass filtering funnels alternately, as one placed below or raised above the level of the other which should be kept stationary. These two filtering funnels are supported on iron rings with clamps which can be attached to two iron rods at any given height. The iron rods should be about thirty-six inches high being held in their places by a framework that rises on the back part of a wooden platform which rests upon the table (or may be built into the wall, or placed between two shelves far enough apart in a closet in the laboratory) and serves as a convenient place for any accessory apparatus or instruments. The funnels, referred to above, are connected at the bottom of each by a rubber tube, and have stop-cocks above the points where the tubing is attached. The opening at the upper part of these funnels is closed with rubber corks through which a glass tube is placed after having been once bent at right angles.

The means of securing the operation of the currents of air in such a manner as to perform their works properly is an arrangement of three-way-stop-cocks of a peculiar construction. These stop-cocks are each so constructed that when turned in one position they have the form of a regular spigot, while by a quarter turn from that position, a straight connection is made through the body of the stop-cock to a tube opening at that point.

Two of these stop-cocks are placed, one on either of the upward branches of a glass tube having the form of the letter Y. This

arrangement affords the means of transmitting a current of air under pressure through that stop-cock which may be turned so as to open a straight connection through it, when that branch of the Y-tube of which it forms a part has been connected with the glass tube at the top of the filtering funnel, from which the air is forced by the flow of the mercury to it from the other funnel which should occupy a higher position. It also affords the means of allowing a current of air to flow to the surface of the mercury in the higher vessel, through the other stop-cock which should be turned so as to afford ingress to the air; this will be secured when the stop-cock



A Constant Pressure Injection Apparatus.

is placed in a position at right angles to that of the first described stop-cock. This branch of the Y-tube should be connected with the tube at the top of the other filtering funnel.

The lower branch of the Y-tube should be connected with the short arm of the bottle containing the injection mass. All the joints made with the tubing should be securely wired and all the other connections made as tight as possible.

The Y-tube described above should be securely supported by extension test-tube clamps, attached to the iron rod, to which the stationary filtering funnel has been fixed, and below the funnel.

The long arm of the bottle containing the injection mass should be attached to the nozzle-stop-cock which fits the canulæ to be used in the injection.

Now we shall suppose that the mercury has been placed in the stationary filtering funnel and this latter firmly fixed a little above the middle point of one of the iron rods. The stop-cock of this funnel should be turned so as to prevent the mercury flowing from it, then the three-way-stop-cock connected with it should be turned so that the air may have access to the surface of the mercury, and the other three-way-stop-cock turned so that the air from the funnel with which it is connected will flow directly through the glass-tube to the wolffs-bottle.

Then if the second filtering funnel be placed in a lower position than the first, above described, and clamped securely, the machine is ready for use.

If the mercury should then be turned on it will force the air out of the lower filtering funnel and this air will then act upon the injection mass, forcing it along in proportion to the difference in level of mercury, or the amount of mercury turned on.

If a larger quantity of injection mass is required than can be discharged from the wolffs-bottle by once emptying one funnel into the other, after the flow of mercury has been stopped by turning the stop-cock of the lower funnel, an additional quantity of the fluid can be poured through the central mouth of the wolffs-bottle.

The positions of the three-way-stop-cocks should now be reversed to permit the current of air to act from the other funnel upon the injection mass.

Then the funnel which is full of mercury should be raised to a position on its rod as much above that of the stationary funnel as it was below it before, when it will in its turn become the source of power when the mercury is turned on.

Thus the operation may be proceeded with, and by repeating this simple process an injection may be performed lasting any reasonable time, with very little trouble or interference on the part of the operator and, so far as my experience goes, with great satisfaction.

It will be readily seen that absolute constancy of pressure is not even obtained in this instance because the shifting lines of level of the surface of the mercury, in the firmly fixed funnels, introduce a

small error: this might be overcome by suspending the funnels from springs so adjusted that as the mercury changed its level, a compensation would be brought about by a change in the position of the funnels; but after having tried the firmly fixed funnels and not having experienced the slightest difficulty from this source of error, I am inclined to believe that a change would be an almost needless refinement of accuracy and in addition would somewhat doubt the prudence of having such a weight of mercury suspended in that manner.

The piece of apparatus has been thus described, somewhat at length in the hope that histologists will find in it an aid in their investigations such as it has been to myself.

A Filtering Wash-bottle especially adapted to the use of the Histologist. By C. E. Hanaman, of Troy, N. Y.

[ABSTRACT.]

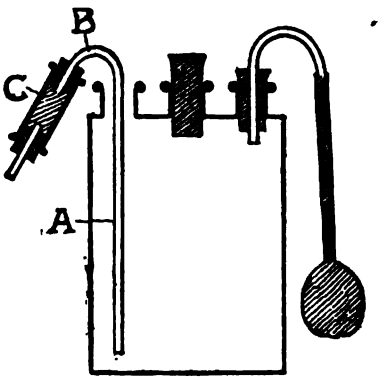
It is a matter of every-day experience, with those who employ the microscope in histological research, that however careful they may be to filter their reagents before the day's work begins, ere long, particles of foreign matter are apt to make their appearance in the various solutions employed.

These particles not unfrequently become attached to the most interesting portions of valuable specimens, thereby marring their beauty, and sometimes detracting from their value. I have not found the usual forms of paper and cotton filters as convenient as I could wish, and it was with a view to the production of a simple apparatus, which would deliver as required any quantity, from a single drop to a full dish of any staining fluid or other reagent, that I devised, some two years ago, the apparatus, a description of which is the purpose of this paper.

A bottle with three necks is taken and through a cork in one of the necks is passed a bent glass tube (A) reaching nearly to the bottom (as in the ordinary wash-bottle) but cut off at the upper end a short distance beyond the bend or neck (B). At this point

there is attached by a cork, a short piece (say three inches long) of a tube of wider lumen (C) than that used in the long tube (A). Into the open or lower end of the wide tube, is inserted a cork, through which passes a short delivery tube.

Over the corks inserted in either end of the wide tube, and covering the ends of the smaller tubes where they open into the larger one, are stretched pieces of fine muslin, and between the two corks the tube is filled with a rather loosely packed plug of "absorbent cotton." The second or middle neck of the bottle is closed by a well fitting cork. Through a cork in the third neck is passed a piece of bent glass tubing, which reaches just below the cork on the inside of the bottle, and extends (either in the form of an arch, or bent at right angles) a short distance from the top



A Filtering Wash-Bottle.

of the cork through which it passes. Attached to this tube is a piece of rubber tubing of a suitable length, at the end of which, either a short glass mouth-piece, or the rubber bulb of a Davidson syringe is attached. To use the apparatus the bottle is filled, through the middle neck, half or two-thirds full of the reagent to be employed and the cork is again firmly inserted. It will then be found, that by compressing the rubber bulb, or blowing through the mouth-piece, the air in the bottle will be condensed and the fluid will be driven through the cotton plug, either in drops or in a stream, according to the quantity of air forced into the bottle. A very gentle squeeze of the bulb, or a gentle breath at the mouth-piece, and a single drop is delivered, while by rapidly condensing the air with the bulb, a stream will flow from the delivery

tube, which can be instantly stopped when the desired quantity has been obtained, by simply loosening the cork in the middle neck. When the plug of cotton becomes foul, as it will after a time, the cork in the lower end of the wide tube is taken out and the plug pulled out with a bit of bent wire, and a fresh one substituted.

The same plug may be used for a long time with most reagents before renewal is required. With reagents like $\frac{1}{4}$ per cent or $\frac{3}{4}$ per cent salt solution, in which the strength of the solution would be altered by condensation in the cotton plug, a daily renewal may be necessary, but with even so troublesome a reagent, as the Hæmatoxylin staining fluid, the plug will not require renewal oftener than once or twice a week, when used daily.

The convenience of the apparatus will I think be apparent to all, and I hope may prove to be as great a source of comfort to others as it has been to myself.

Development of the Planula of Clava leptostyla, Ag. By J. H. Pillsbury of Springfield, Mass.

[ABSTRACT.[1]]

After referring to the habitat of this hydroid the author gave a brief description of the familiar method of its development from the formation of the germinal dot to the escape of the planula from the sporosac. The special point to which the author wished to call attention is the fact that the cilia are developed before the planula bursts forth from the sporosac, a fact which has been either overlooked or denied by previous observers, and the probable service the ciliary motions render in accelerating the escape of the planula.

[1] Published in full in the American Monthly Microscopical Journal, for October, 1882.

TITLES OF OTHER PAPERS READ IN SECTION G.

ON A NEW FORM OF DRY MOUNTING. By Wm. A. Rogers of Cambridge, Mass.

A STUDY OF THE PROBLEM OF FINE RULING WITH REFERENCE TO THE LIMIT OF NAKED EYE VISIBILITY AND MICROSCOPIC RESOLUTION. By W. A. Rogers of Cambridge, Mass.

ON THE EPIDERMIS OF MARSIPOBRANCHS. By Albert H. Tuttle of Columbus, Ohio.

THE HOUSE-FLY CONSIDERED IN RELATION TO CONTAGIOUS POISONS. By Thomas Taylor of Washington, D. C.

A NEW FREEZING MACHINE FOR SECTION CUTTING. By Thomas Taylor of Washington, D. C.

DEMONSTRATION OF THE BACILLUS OF TUBERCULOSIS. By William Osler of Montreal, Canada.

SECTION H.

ANTHROPOLOGY.

ADDRESS

BY

DANIEL WILSON, LL.D., F. R. S. E.,

VICE PRESIDENT, SECTION H.

SOME PHYSICAL CHARACTERISTICS OF NATIVE TRIBES OF CANADA.

In welcoming the Anthropological Section of the American Association for the Advancement of Science to its meeting here on Canadian soil, it will not, I trust, appear unsuitable to the occasion, if I invite attention to some of the physical characteristics which distinguish certain native races of the Dominion; and especially to the significance of certain typical head-forms, and their bearing on our special researches in reference to the origin, distribution, and classification of races.

In so doing, it is important to keep in view the prevalence throughout the American continent of various artificial modifications of skull-forms. This strange custom is probably at the present time carried on more systematically among the different tribes of Flathead Indians of British Columbia, than in any other region; though abundant evidence exists to show its prevalence both in past and present times among many tribes and nations in very different stages of progress, alike in North and South America. It has, indeed, attracted more general attention than most other characteristic practices of the American aborigines, owing to its prevalence alike among the most barbarous and the most civilized races. To all appearance the Peruvians and Mexicans had devel-

oped independent phases of progress in arts, science, and social policy, without any knowledge of each other. Nevertheless, we trace the singular practice of moulding the human head into abnormal forms, alike among the civilized races of Peru, the ancient lettered architects of Central America and Mexico, and among barbarous tribes both to the east and west of the Rocky Mountains. The earthworks of the Mississippi Valley Mound-builders have been found to cover artificially flattened crania; and the student of American native civilization, as he turns from pondering over the bas-reliefs and hieroglyphics on the sculptured slabs of Palenque and Uxmal, is startled to find that the cranial forms and strange physiognomical contour of the architectural race of Central America are reproduced among some of the most barbarous living tribes of Oregon and British Columbia. But, now that the study of craniology has been carried out by many intelligent observers, the fact is becoming familiar to us that artificial cranial deformation is no peculiarity of the American continent, either in ancient or modern times. The compressed crania of the Asiatic Macrocephali attracted the attention of Hippocrates five centuries before the Christian era; and Blumenbach, the foremost of European craniologists, figured in the first fasciculus of his "Decades Craniorum," in 1790, an imperfect compressed skull, received by him from Russia, with the information that it was probably that of a Tartar. This he unhesitatingly designated an Asiatic Macrocephalus. The conclusion thus arrived at has been sustained by subsequent discoveries; and as attention is more widely directed to the general subject the results are found to have a special value for the American ethnologist.

It seems probable that the name of Macrocephali, like that of our own Flatheads, did not properly belong to any single tribe, or even distinct race of ancient Asia; but had its origin in the effort, by artificial means, to produce the patrician head-form, primarily characteristic of some dominant, or conquering race. Among the Chinooks and other Flathead tribes of this continent, and also, as I believe, among the ancient builder-races of Yucatan and Peru, certain head-forms were recognized as an attribute of the ruling cast. Within the Flathead area of British Columbia the compressed and distorted skull is even now the symbol of aristocracy; and adopted captives, or slaves, are precluded from giving the prized deformity to their offspring. Hippocrates refers in

his "De Aere, Aquis, et Locis," to the Macrocephali as a people among whom "those are thought the most noble who have the longest heads." Skulls of this type have been recovered in recent years from ancient graves in the Crimean Bosphorus, and the valley of the Don. Still more illustrative of the effort at superinducing a novel dolichocephalic form among races of brachycephalic type, are the examples of compressed Hun or Avar skulls found from time to time on the line of march of the great Hunish invasions of Europe in the fifth and sixth centuries. One of the first examples of such mediæval compressed crania which attracted special attention in Europe was a skull found, in the year 1820, at Fuersbrunn, near Grafenegg, in Austria. Count August von Breuner, the proprietor of the soil, acquired possession of the interesting relic, and at once ascribed it to the Avarian Huns, who occupied that region from the middle of the sixth until the eighth century. Of this compressed Avar skull, Retzius gave a description in the proceedings of the Royal Academy of Sciences of Stockholm, in 1844; and showed that the skull, which had been regarded as remarkable for its great elongation, was in reality a true brachycephalic skull, such as the Mongol affinities of the Avars would suggest, but that by artificial compression it had been elongated, vertically, or rather obliquely. An additional interest is conferred on this European example of artificial cranial deformation by the fact that scientific observers were persuaded for a time to regard it, not as European, but as an intrusive American example, brought thither soon after the discovery of this continent. The well known traveller Dr. Tschudi communicated to Müller's "Archiv für Anatomie" a memoir, in which he instituted a comparison between this Grafenegg skull and the compressed crania of ancient Peruvian cemeteries, whence he arrived at the conclusion that scientific men had been deceived in ascribing to any Asiatic or European source a skull which must have been originally derived from Peru. In confirmation of this, he recalled the fact that, widely as Austria and Peru are now severed, in the sixteenth century the Emperor Charles V embraced both within his dominions. He accordingly conceived it no improbable conjecture that the compressed skull was brought to Europe, as an object of curiosity; and being afterwards thrown aside, it was mistakenly assumed to pertain to native sepulture when recovered at Fuersbrunn in the present century.

More recent discoveries of artificially compressed crania on European sites, have removed all doubts of their native, or intrusive Asiatic origin. It thus appears that the barbarous practice is neither recent, nor peculiar to the New World. Neither to America nor to Europe do those examples of mediæval and ancient compressed crania really belong, but seemingly to the nomad Mongols and Ugrians of the steppes of Northern Asia, in the vast wilds of which we lose them as they spread away eastward toward the Okhotsk Sea and the Aleutian Islands. We are thus guided by unmistakable indications backward, as it seems, on this ancient trail, down the valley of the Danube, and beyond the Caspian and the Ural Mountains, to a region outside the farthest limits assigned by Hippocrates, Strabo, Pliny, or Mela, to the Asiatic Macrocephali; and recover traces of the strange practice of the American Flatheads far to the northeast of the Altai chain, in the valleys that skirt the Yablonoi mountains, as they trend eastward towards the Okhotsk Sea. It may indeed be an American practice which Asia borrowed; for the affinities of race between the tribes of the islands and Asiatic mainland immediately to the west of Behring strait point to a migration to Asia from America. Such, however, is limited and exceptional. On evidence which embraces the ethnical characteristics of a very wide Asiatic area, the Mongolian classification of the American Indian is confirmed by many significant points of resemblance in form, color, texture of hair, and peculiar customs and traits of character, which fail us when we turn either to the Asiatic Aleutians, the Namollos, and other allied tribes of the older continent, or to the true Eskimo. The striking resemblance noted by Humboldt as existing between the American race and the Mongols of Asia, received independent confirmation from Dr. Charles Pickering, as the result of his extensive observation of the races of both continents, in his capacity of ethnologist to the American Exploring Expedition. Such affinities are still further confirmed, as we recover the traces of the singular practice of cranial deformation extending in ancient and mediæval times eastward from the Euxine and beyond the Altai mountains. To those little-known areas of northern Asia the ethnologist and the archæologist have yet to turn in quest of the footprints of one of the immigrant routes to the new world. There it is, in the vast unknown regions of Asiatic Russia, that we may hope to recover evidence confirmatory of at least one source of the Asiatic relations of the American race.

It is now a recognized fact that the artificial head-forms characteristic of diverse tribes of North and South America vary greatly, from the extreme depressed forehead and laterally compressed skulls of races that rivalled the ancient Macrocephali in their estimation that "the most noble are those who have the longest heads," to some among the Cowlitz or Chinook tribes of British Columbia, whose heads are compressed into a flattened disk. The two artificial extremes find their analogues in the distinct ethnical divisions of dolichocephalic and brachycephalic head-forms among well-known northern tribes. The predominant natural form, characteristic of the more southern tribes of North America, appears to have been brachycephalic, or, as it is sometimes called, globular. But along the regions of the great lakes, in the valley of the St. Lawrence, and northward throughout the whole Eskimo area, the dolichocephalic head-form prevails. The native races of the Dominion, and especially the earliest known aborigines of Upper and Lower Canada, including the province in which you are now met, appear to have been all of the same dolichocephalic type; and so to have formed a class markedly distinct from the short, or globular headed races of the south, whose head-form was long regarded as typical of the whole American race. Of the Indians of Hochelaga, first met by Cartier, in 1535, we are able to judge from crania recovered from their cemeteries. The palisaded Indian town of Hochelaga occupied, in the sixteenth century, the site where we are now assembled; and in the museum of McGill College may be seen examples of the crania, as well as specimens of the flint implements and pottery dug up on its site. Its traces revealed nothing suggestive of any other rudiments of civilization than have long been familiar to the American student of primitive arts in the abundant remains of Indian settlements throughout the area of the eastern States, and on the sites of the Iroquois Confederacy in the State of New York. Their earthenware pots and bowls of various sizes were decorated with rude yet tasteful incised patterns; and the handles were further ingeniously modelled at times into human and animal forms. Tobacco pipes also, both of stone and earthenware, here as elsewhere, were special objects of artistic ornamentation. Stone and flint implements, bone needles and bodkins, also abounded; but of metal only very rare traces of the cold-wrought copper tool gave any indication of even the first rudiments of metallurgic art. In truth, Canada has no such evidences, even of

an incipient native civilization, as the remarkable earthworks which abound in the great river valleys to the south of Lake Erie. To all appearance, through unnumbered centuries, the tide of human life has ebbed and flowed, to the north of these great lakes, and in the valley of the St. Lawrence, as unprogressively as on the great steppes of Asia, among the Bedouin tribes of the Arabian peninsula, or around the tropical lakes of equatorial Africa. Such footprints as the wanderers have left on the sands of time tell us no more than the ripples on the sea beach, and are indeed still more evanescent. Nevertheless, in all their distinctive characteristics, the tribes of our Canadian forests and prairies present much in common with those by whom the whole area of this northern continent, southward to the Gulf of Mexico, appears to have been occupied when first brought under the notice of European explorers.

It is indeed a noticeable fact in reference to the entire population of this western hemisphere, throughout areas so widely differing in climate and physical geography as are embraced within the region extending from the arctic circle to Terra del Fuego, that the ethnical diversities are slight when compared with those which pertain to what, historically speaking, are the older continents. It seems to force on us the conclusion that, however remotely we may trace our way back into unrecorded centuries, ere we reach the time when man made his first appearance here, so far as the multiplication of diverse racial varieties afford any evidence, it is recent when compared with the peopling of the ancient world. To this indeed one important exception has been suggested in the assumption of a direct affinity between the hyperborean tribes of this continent and the men of Europe's palæolithic era; and I shall accordingly refer to it in its bearings on the general conclusions to which we are thus led.

Great, however, as is the superficial resemblance which seems to pervade the diverse tribes of the American continent, some of the underlying differences were noted from the first. Columbus, with an eye quick to discern all that was peculiar in the novel scenes on which he was the first to gaze, failed not to note the marked distinction between the fair complexion of the Guanches, who were brought under his notice on his first voyage, and the reddish-olive of the ferocious Caribs. Apart from this purely physical distinction, these Guanches attracted his attention by their gentle manners and inoffensive habits. From them he learned

of the Caribs, as a fierce, warlike people occupying the neighbouring islands and the mainland, of whom they lived in constant dread; and who subsequently became familiar to the Spaniards as a ferocious, crafty, and revengeful race, delighting in cannibalism. Hence we perceive that from the first both physical and moral differences, of a sufficiently marked character, were observed among native tribes of the New World. Nor indeed did Humboldt, or even Morton, entirely overlook the existence of considerable varieties in color and complexion, from nearly white to a dark brown; though they were led, from different causes, to underestimate the extent of diversity prevailing among the widely scattered nations of North and South America.

But while it is deserving of notice that the aborigines of Canada do differ in certain physical characteristics from those especially of the more southern states of North America, it is undoubtedly true that an approximate correspondence in ethnical characteristics is common to many tribes both of North and South America. It is not, therefore, to be wondered at that the idea of their constituting one native stock distinct from all the races of the Old World, and agreeing in the possession of physical characteristics peculiar to themselves, should have been accepted for a time as indisputable. The vague generalizations of travellers, and the current forms of popular belief, however, gradually acquired consistency as an accepted canon of ethnical science; until, in the final embodiment of Dr. Morton's matured opinions, he affirmed the American race to be essentially separate and peculiar, and with no obvious links, such as he could discern, between them and the people of the old world, but a race distinct from all others.

The geographical facilities for intermixture among the very diverse races of Asia, Africa, and Europe, account for many intermediate and transitional races; but this increases rather than diminishes the difficulty of referring to any satisfactory source, such primary types of extreme diversity as the Negro, Berber, Mongol, Malay, Arab, and Saxón. Here, on the contrary, so far as now appears, approximate types were hemmed in between the Atlantic and the Pacific; and in so far as they intermingled, the tendency necessarily was to diminish, if not to efface, any strongly marked distinctions; just as, in prehistoric centuries, the blending of the aboriginal savage with intruding races is assumed to have begot-

ten the Melanocroi and the Xanthocroi of Europe's ethnological classification.

Here, undoubtedly, as well as in Europe and Asia, extreme diversities have been modified; but from the first these differences must have extended over a narrower range on the American continent than that which finds such curious illustration in the ancient sculpture and paintings of the Nile valley. The baso-relievos of Yucatan, the terra cottas of Mexico, and the pottery of Peru, furnish analogous evidence of considerable diversity of type among the prehistoric, as well as the historic and civilized races of the New World. Nevertheless, after the fullest recognition of all that such evidence indicates, the fact remains that great as is the divergence of the Eskimo from the Mexican, or the Peruvian from the Patagonian, the difference becomes almost insignificant in comparison with that which distinguishes the Aryan Hindo from the Andaman Islander, the Arab from the Chinese, or the insular Malay from the Negritto. Yet all of those pertain to a continent which is only separated from our own by Behring Strait. So noticeable indeed is the prevailing correspondence in ethnical characteristics among the various races of this continent, that the elements of diversity were long overlooked, even by acute scientific observers. Malte Brun affirmed as the result of a long course of observation, "that the Americans, whatever their origin may be, constitute at the present day a race essentially different from the rest of mankind." A more notable authority, possessed alike of rare capacity for accurate discrimination, and of opportunity for extended personal observation,—the distinguished scientific traveller, Humboldt,—remarked in the preface to his "Researches:" "The nations of America, except those which border the polar circle, form a single race, characterized by the formation of the skull, the color of the skin, the extreme thinness of the beard, and the straight glossy hair."

Until very recent years this was accepted as no less indisputable than any axiom of Euclid. American ethnologists were agreed as to the predominance of one ethnical type throughout the whole western hemisphere; while those of Europe, with rarer opportunities for personal observation, were predisposed by all the narratives of early voyagers to accept the conclusion that the man of the New World was a well-defined variety, if not a distinct species, of the genus *Homo*. Prichard, Lawrence, Wiseman,

Knox, Morton, Agassiz, Squier, Gliddon, Nott, and Meigs, might each be quoted in confirmation of this opinion, and especially of the prevailing uniformity of certain strongly-marked cranial characteristics. Agassiz, for example, affirmed in very explicit language: "with the exception of the Arctic Esquimaux, there is only one single race of men extending over the whole range of North and South America, but dividing into innumerable tribes; whilst, in the Old World, there are a great many well-defined and easily distinguished races, which are circumscribed within comparatively much narrower boundaries." Morton, again, viewing the subject in the light of his own special evidence, designated a markedly brachycephalic skull, with flattened occiput, recovered from one of the mounds in the Scioto Valley, "an aboriginal American head," and added: "this is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection." Accordingly, after indicating these in detail, he affirms: "it is the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate."

Among what may be designated typical Canadian skulls, those of the Hurons of the region lying around the Georgian Bay have a special value. They represent, as we believe, a native race which, under various names, extended from the Lower St. Lawrence westward to Lake St. Clair, the *Ouane-doté:* including the Petuns, Neuters, Hurons, Eries, and other Wyandot tribes, of the same stock as the Iroquois; but to whose implacable enmity their extermination was ultimately due. The native population first met with by Cartier and the French explorers of 1535, is believed to have been of the same Wyandot stock; but before the return of the French under Champlain, in 1603, they had been exterminated, or driven westward to the later country of the Hurons, on the Georgian Bay. There they were first visited by Champlain in 1615, and subsequently by the French Jesuit missionaries who, in 1639, found them occupying thirty-two palisaded villages. Brébœuf reckoned their number in 1635 at thirty thousand, and they are estimated, in the "Relation" of 1660 at thirty-five thousand. Already, at that early date, the whole country westward from the Ottawa to the Huron country around Lake Simcoe, had been depopulated, and reduced to a desert, by the wrath of the Iroquois. Charlevoix assigns the year 1655 as that of the destruction of

the Attiwendaronks, or Neuters, who occupied the fertile Niagara peninsula between Lake Erie and Lake Ontario; and the Eries, whose name is perpetuated in the great lake on whose shore they dwelt, had already been exterminated by the violence of the same kindred race, before the French explorers had even ascertained the existence of the lake which bears their name. In the earlier French maps an imaginary river extends uninterruptedly from Lake Huron to Lake Ontario.

Minute information has been preserved of the Hurons in their later home, derived chiefly from the "Relations" of the Jesuit Fathers, communicated to the Provincial of the Order at Paris, from 1611 to 1672. We thence learn accurate details of their great "Feast of the Dead" celebrated at intervals of ten or twelve years, when the remains of their scattered dead were gathered from old scaffolded biers, or remote graves, and deposited with grand ceremonial and mourning in the general cemetery of the tribe. In the vicinity of the sites of their palisaded villages extensive ossuaries have repeatedly been found; and there are now preserved, in the museum of Laval University at Quebec, upwards of eighty skulls recovered from the Huron cemeteries of St. Ignace, St. Joachin, St. Mary, St. Michael, and others of the Huron villages, so designated by the French Missionaries who visited them in the seventeenth century, and labored for the conversion of the Indians there. Other examples are preserved in the museum of Toronto University; and I may add that Dr. Taché, by whom the most extensive researches were carried on, presented ten Huron skulls to the London Anthropological Society; and I have since forwarded specimens to Dr. De Quatrefages for the museum of the Jardin des Plantes at Paris.

A special value attaches to the skulls recovered from those Huron ossuaries, from the fact that the race was exterminated, or driven out of the country, by their Iroquois foes, in 1649: and hence the crania recovered from their old cemeteries may be relied upon as giving a fair illustration of the physical characteristics of the race. The descendants of a small band of Huron refugees, rescued from the general massacre, and brought by the French missionaries to Quebec, still survive at the Huron village of Lorette, on the St. Charles River; but they have long since lost the pure traits of full-blood Indians, and are chiefly interesting to the ethnologist now from the evidence they give of the

survival alike of native intellectual and physical traits, after an interval of well nigh two centuries and a half passed in intimate intercourse, and latterly frequent intermarriage with the French habitans.

The Huron skull is strongly marked as of the dolichocephalic type. The careful measurements of thirty-nine male skulls yield a mean longitudinal diameter of 7.39 to a parietal diameter of 5.50; and of eighteen female skulls, a longitudinal diameter of 7.07 to a parietal diameter of 5.22. One essential characteristic, therefore, that of great relative length, is unmistakable. I specially refer to this now, because we possess, in the collection of the Canadian Institute at Toronto, a skull recovered from one of the Huron ossuaries near Lake Simcoe which differs essentially from this Huron type. It is a short skull,— shorter even than that from the Scioto mound,— of the same, so-called, globular type, measuring only 6.00 in longitudinal, and 6.40 in parietal, diameter. Reverting, therefore, to Dr. Morton's ascription to the Scioto mound skull of national characteristics, which constitute it "the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate," this northern example, if it stood alone, would seem to confirm his assumption. But it is a wholly exceptional case; so distinct from the true Huron type that, after a careful study of one hundred and twenty-six crania from ossuaries of the Huron country, including considerable deviations from what may be regarded as the normal type, I have not found one other example approximating to it. It differs little less essentially from the race-form of the people whose grave its owner shared than that of a Chinese from the normal skull of the pure Anglo-American; and may be assumed as that of an Indian belonging to some far southern tribe, whom the chances of Indian warfare had made a captive, or an adopted member, of the Huron tribe in whose cemetery he found his final resting-place.

Such indications of physical diversity, among the nations so widely scattered throughout the New World, accord with philological and other evidence. Not by one, but by diverse routes have the fathers of the American nations found their way thither: some by Behring Strait and the Aleutian Islands; others by more southern routes across the broad Pacific, aided by winds and currents, and passing onward from island to island of the great archipelago; others, as we know, by Iceland and Greenland, across

the northern Atlantic; and others again—as philological evidence seems to indicate,—along the same route as that which Columbus successfully pursued in 1492. But to the primary migrations we know not how remote a date to assign, in order to allow of the interblending of intruding races, and the development of the native American "Red Man" with all his distinctive traits of individuality. For, while it is important to note the elements of diversity, it is nevertheless true that the New World does differ from the Old in the narrow range of such variations of race-type through all extremes of climate from arctic to temperate, tropical, and antarctic. The European traveller who surveys his own continent from the northern habitat of the Fins and Lapps, and the corresponding Asiatic hyperboreans, and then traverses the eastern hemisphere to the Cape or to the Indian Ocean, comes in contact with all intermediate varieties between the two extremes of the white and black races; and recognizes in western Europe the Melanocroi who seem to be the resultant of their inter-blending in prehistoric times. But in America we seem to see no more than a result analogous to the latter; and this as the product of more nearly allied primitive stocks, the largely preponderating element of which has been derived from the Mongol area of eastern Asia. Philological evidence, on the other hand, no less clearly indicates the remoteness of the migrations by which this first colonization of the New World was effected; it may be, indeed, that they pertain to periods when the physical geography of both continents, and of the intermediate archipelago, afforded facilities for migration altogether wanting within historic times.

But such ideas of a derivative origin of the American aborigines are of very modern growth, and are only now displacing long accredited beliefs. That the man of this New World must prove a being essentially different from any known race of Europe, Africa, or Asia, was an opinion which assumed ever stronger confirmation, as the idea of Columbus that he had landed on the eastern Continent faded away from the minds of his successors. The Indians of his new-found world were no natives of Cipango, or the valley of the Indus; and the literature of the fifteenth and sixteenth centuries abounds with evidence that it was much easier to persuade the men of that age that Calibans and monstrous Anthropophagi peopled the strange regions beyond the Atlantic, than that these were inhabited by human beings like themselves.

Even Columbus, it has to be remembered, in searching for evidence to confirm his own scientific demonstration that the world was a sphere, and so, that the eastern continent could be reached by a western route, attached special value to indications of the existence of a transatlantic continent, derived from the fact that the bodies of two dead men had been cast ashore on the island of Flores, differing essentially in features and physical characteristics from any known race. When, at length, the great discoverer set foot on the islands first visited by him, the peculiarities which marked the gentle and friendly race of Guanahanè were noted with curious minuteness; and their "tawny or copper hue," their straight, coarse, black hair, strange features, and well-developed forms, were all recorded as objects of interest. On his return, the little caravel of Columbus was freighted not only with gold and other coveted products of the New World, but with nine of its natives, brought from the Islands of San Salvador and Hispaniola, eight of whom survived to gaze on the strange civilization of Spain, and to be themselves objects of scarcely less astonishment than if they had come from another planet. Such was the earliest knowledge acquired by the Old World of the type of humanity generically designated as the Red Indian; and the attention which its peculiarities excited when thus displayed in their fresh novelty has not yet exhausted itself, after an interval now little short of four centuries.

Of all known races of the New World, the Eskimo alone presented, at first, a seemingly marked diversity from the other aborigines; though the grounds on which such a conclusion was based are traceable far more to Arctic conditions of life, than to any ethnical peculiarities definitely assigned to them.

This is apparent from the terms employed by the historian Robertson, who, writing in 1777, says: "The Esquimaux are manifestly a race of men distinct from all the nations of the American continent, in language, in disposition, and in habits of life. But among all the other inhabitants of America there is such a striking similitude in the form of their bodies, and the qualities of their minds, that, notwithstanding the diversities occasioned by the influence of climate, or unequal progress of improvement, we must pronounce them to be descended from one source."

The idea thus definitely set forth by the Scottish historian of the last century was placed on what seemed to be a strictly scientific basis by the author of the "Crania Americana." Dr. Morton's dili-

gence in the accumulation of evidence merits our highest gratitude. But he had unfortunately set out with the idea of one nearly uniform race peculiar to the New World; and with all the evidence before him which has since sufficed to convince others of the prevalence of great diversity of head-forms among American races, he persisted to the close in maintaining the physical unity of the Red Race from the arctic circle to Cape Horn.

Of the three propositions with which Dr. Morton sums up the results borne out, as he conceives, by all the evidence advanced in his "Crania Americana," one is, "That the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character." Any difficulty arising from physical or other differences, he sought to overcome by the application of the hypothesis that "these races originated in *nations*, and not in a single pair; thus forming proximate but not identical species." Subsequent to his death his collection of crania was acquired by the Academy of Natural Sciences of Philadelphia, and thereafter supplemented in its most important branches by many valuable additions. This greatly augmented collection was classified anew and catalogued by the late Dr. J. Aitken Meigs; and after having thus had all the evidence which it presents brought in systematic order under his own notice, he contributed to Gliddon's "Indigenous Races of Men," a paper entitled "Cranial characteristics of the Races of Men," in which he thus reiterated the Mortonian dogma: "Through *Crania Americana*, it has long been known to the scientific world that a remarkable sameness of osteological character pervades all the American tribes from Hudson's Bay to Terra del Fuego. It is equally well known that the researches of Humboldt and Gallatin have demonstrated a conformity not less remarkable in the language and artistic tendencies of these numerous and widely-scattered aborigines."

The artificial transformations of the Indian head were at first a source of difficulty in any systematic classification of head-forms; and the views of Dr. Morton underwent considerable modification on some points relating to the influence of this custom in perpetuating certain types of head; but he finally reverted to the original idea of one predominant cranial type to which all the races of the American continent more or less nearly approximated.

The evidence which the mediæval European examples of cranial

deformation supply suggests the origin of this barbarous practice in an aim at conformity with the natural head-form of a patrician or conquering race. Dr. Fitzinger, who has carefully investigated the whole subject of the discovery of macrocephalic skulls in ancient Austrian sepulchral deposits, after tracing the evidence supplied by the allusions of classic writers, mentions an interesting independent illustration of the subject. A medal, struck apparently to commemorate the destruction of the town of Aquileia, by Attila the Hun, in the year 452, came under his notice. On one side is represented the ruined city, and on the other the bust of the Hunish leader in profile, with the same form of head as that shown in the supposed Avar skulls found in the valley of the Danube. One of this type obtained by M. Hippolyte Gosse, from an ancient cemetery in Savoy, presents the favorite Hun or Avar form when viewed in profile, with the singular vertical elongation which appears to have constituted an ideal type of masculine beauty among the Asiatic followers of Attila, as among the Natchez, the Peruvians, and other nations of the New World. It was found at Villy, near Reigner, and has been engraved by Retzius, from a drawing furnished to him by the discoverer.

Thierry, in his "Attila," refers to the artificial means resorted to by his followers in order to give a Mongolian physiognomy to their children. The Hunish leader welcomed every able bodied recruit to his standard, and was in reality as much a leader of Goths as of Huns; though the black Huns from the dreary Siberian steppes constituted the aristocracy of his wild followers, whose Mongolian physiognomy formed the ideal of ethnic beauty. At this the Gothic mother accordingly aimed, by bandaging the nose, compressing the cheek bones, and giving an artificial form to the cranium of her infant. Such practices, however, when once brought into general use, continue long after the reason for their adoption has ceased. It need not therefore greatly surprise us to learn that the practice of distorting the skull in infancy still prevails in some districts of France. Among the examples of such cranial malformation engraved by Dr. Foville, in his work on the "Anatomy of the Nervous System," there is one which might take its place alongside of some of the most exaggerated specimens brought from Peruvian cemeteries.

But however the Gothic mother might labor to make the natural development of her infant's head conform to the Mongolian model,

the traces of the originally dolichocephalic type could not be wholly eradicated. This is seen on comparing examples in any large collection of American Indian skulls. In the compressed and distorted Peruvian crania, traces of two distinct types appear to me still uneradicated. The same is noticeable in the sculptures of Central America, as in the Palenque bas-reliefs where deities and chiefs treading kneeling figures underfoot, present the long, sloping forehead in a line with the straight nose, with other features of the strange profile peculiar to the old race; while the subject race is hook-nosed, with high foreheads, and heads seemingly uncompressed. If the idea is well-founded, which thus traces the origin of this barbarous practice to the efforts of an inferior, or subject race to approximate in outward appearance to the privileged class, its very occurrence points to the existence at some previous time of races essentially diverse in physical character. And if we assume their relative positions to have been akin to that of the conquering Hun and the enslaved Frank or German, the motive to such a practice is sufficiently obvious. Were it possible for the colored population of Canada and the United States, at the present day, by any analogous process to assimilate their offspring to the Anglo-Saxon type, how irresistible would the motive be to its use.

Such ideas, however, found no favor with the author of the "Crania Americana;" and in some of the conclusions finally adopted by him he has been sustained by authorities of just weight in science. In the latest record of his matured views, as set forth in a posthumous paper contributed to Schoolcraft's "History of the Indian Tribes," he remarks: "I at first found it difficult to conceive that the original rounded skull of the Indian could be changed into this fantastic form; and was led to suppose that the latter was an artificial elongation of a head remarkable for its length and narrowness. I even supposed that the long-headed Peruvians were a more ancient people than the Inca tribes, and distinguished from them by their cranial configuration. In this opinion I was mistaken. Abundant means of observation and comparison have since convinced me that all these variously formed heads were originally of the same rounded shape."

In that same final contribution to his favorite science, Dr. Morton's matured views on the whole subject of the cranial type of the American continent—based on the additional evidence accumulated by him, in the interval of twelve years which elapsed

between the publication of the "Crania Americana" and his death,— are thus defined: "The Indian skull is of a decidedly rounded form. The occipital portion is flattened in the upward direction, and the transverse diameter, as measured between the parietal bones, is remarkably wide, and *often exceeds the longitudinal line.*"

It is curious to observe in this latter statement the evidence of a careful and most conscientious observer allowing all the proofs of varying physical type which his own indefatigable industry had accumulated, to be subordinated to this foregone conclusion. Here Dr. Morton must have had in view his theoretical type, rather than the results of his own careful observations, for even if he accepted as evidence the artificially abbreviated and flattened skulls, his "Crania Americana" furnishes only one exceptional example, from a mound on the Alabama river (Pl. LIV), of which he says: "It is flattened on the occiput and os frontis in such a manner as to give the whole head a sugar-loaf or conical form, whence also its great lateral diameter and its narrowness from back to front." The idea had, in fact, received nearly universal acceptance that the European immigrants of the fifteenth and subsequent centuries intruded upon races of wholly distinct origin from themselves, and were displacing the true American autocthones, with whom they had nothing in common.

When Prior Fernando de Talavera of Salamanca summoned a meeting in the Convent of San Estebán, in the year 1487, to take into consideration the proposition of Columbus that the earth was not a plane, but a sphere; and that, by sailing in a western course, land, which he assumed must be the most eastern coast of Asia, would be reached: the assembled philosophers and theologians gravely pronounced the idea of the earth's spherical form heterodox, and a belief in antipodes incompatible with the historical traditions of our faith; since to assert that there were inhabited lands on the opposite side of the globe, would be to maintain that there were nations not descended from Adam, it being impossible for them to have passed the intervening ocean!

We smile at the orthodox philosophers and theologians of the fifteenth century, who, with the help of St. Jerome and St. Augustine, proved this western hemisphere of ours to be an impossibility; yet it is curious to detect the same old prejudices unconsciously influencing the minds of some of the acutest men of science in very recent years. What else was it, if not this "im-

possibility for them to have passed the intervening ocean," or in other words, to have sprung from the same stock, which led one of our own most valued associates, the late Professor Agassiz—a scientific observer of rare sagacity and experience, and one who regarded the entire question of American ethnology from a point of view peculiarly his own,—to adopt the conclusions of Dr. Morton, in spite of the palpable inconsistency of the evidence which he was so well qualified to estimate at its true worth? In his "Sketch of the Natural Provinces of the Animal World, and their relation to the different Types of Man," while appealing to the results arrived at by Dr. Morton, in reference to the imagined unity of the whole American aborigines as "a single race," he reaffirms the homogeneous characteristics and ethnic insulation of the American Indian on entirely novel grounds. After defining the evidence on which his general conclusion is based, that the boundaries within which the different natural combinations of animals are circumscribed on the surface of the earth coincide with the natural range of distinct types of man, he proceeds to show that America, including both its northern and southern continents, differs essentially from Europe and Asia, or Africa, in being characterized throughout by a much greater uniformity in all its natural productions, and of plants and animals as to make it the New World. He then [illegible] these facts before us we may expect that there should be no great [illegible] among the races of man that might [illegible] and which the most evidently distinguished of these peculiarities has led Dr. Morton to adopt [illegible] a single race, [illegible] Indian, which [illegible]

same palæontological zones. 2nd, That whilst all such remains of man are exclusively of the American Indian type, the monkeys called *Hapale*, *Cebus*, *Callithrix*, etc., are equally 'terræ geniti' of this continent. . . Finally, that *permanence of type*, as well for humanity as for simiadæ, is firmly established in both genera, from the hour in which we are living, back to a vastly remote, if not incalculable, era of unrecorded time." To the evolutionist of our own day, the very result of such reasoning is to lead to ideas of unity of origin vastly more comprehensive than that which, within a period so very recent, was thus rejected as wholly incompatible with deductions from much industriously accumulated evidence.

It is curious, indeed, to endeavor now to realize to ourselves what distinct idea was present in the mind of Agassiz when he apportioned his essentially diverse types of man to their specific "natural provinces;" or what Dr. Morton conceived in his own mind when, after affirming one of the three propositions which he believed himself to have established, to be "that the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character:" he fancied that any difficulty, arising from such physical diversities as it was impossible for even him entirely to ignore, could be removed by advancing the hypothesis, that "these races originated in *nations*, and not in a single pair; thus forming proximate but not identical species." The only thing which is at all clear is the assumption of what may be called a gregarious creation: the summoning into existence, by some unknown process, or creative fiat, of an entire race, or nation, at the first peopling of this New World with its own specific aborigines, "essentially different from the rest of mankind."

But Malte Brun and Robertson, Humboldt, Morton, Meigs, Gliddon and Agassiz, all concur in excepting the polar tribes, or Eskimo, from the assumed American race peculiar to this continent. Latham says of the Eskimo: "physically he is a Mongol and Asiatic; philologically he is American, at least in respect to the principles upon which his speech is constructed." But whencesoever we may derive them, they too are ancient and widely scattered occupants of the strange inhospitable region appropriated to themselves. One branch of them, the Labrador Eskimo, borders on our own Eastern settlements on the St. Lawrence: beyond these are the East and the West Greenlanders, including the natives of

the Danish trading settlements. To the north of them are the Eskimo of the west coast, north of Melville Bay, styled, by Sir John Ross, the "Arctic Highlanders." But their extreme northern limits have yet to be determined. The most advanced arctic explorers have either come in contact with the natives, or found traces of their habitation; and their habits and indifference to the extremest rigor of the climate, justify the assumption that only the absence of game will restrict the limits of their habitat. They occupy the whole coast regions of Behring Strait; and extend beyond that to the islands and neighboring continent, westward even to the shores of northeastern Siberia. The collection formed by Professor Nordenskeold in his Vega expedition — part of which was exhibited at Edinburgh during the present year,— includes an interesting series of implements used by the Chukches of Siberia and the Asiatic Eskimo in fishing and hunting. They employ the same kind of harpoon for hunting the walrus; use a long spear of nearly the same fashion, generally furnished now with an iron head, for hunting the bear; while their arrows are still pointed with walrus ivory. Such traces alike of community of arts and of race, within the arctic circle of the Asiatic and American continents, and even extending to Europe, show that, whatever may have been the ancient lines of migration, the overflow in later centuries across Behring Strait has been from the American continent westward into the Old World.

This widely scattered race, though corresponding in ethnical character, is broken up, by the exigencies of their rigorous climate into small tribes and isolated bands, dispersed for the most part over a coast line extending from Labrador to Behring Strait upward of 5,000 miles, and migrating with the animals on which they depend for subsistence. They are hunters and fishers. The deer, the polar bear, the wild goose, swan, and other birds that resort to arctic breeding grounds, are alike objects of the chase; but they primarily depend on seals and cetaceous animals, the blubber of which furnishes food calculated to beget the animal heat which enables them to brave the severity of an arctic climate. *Eskimántzik* appears to be an Abenaki term signifying "eaters of raw flesh;" and as such indicates the surprise with which even the Indian nomads of New England viewed the strange habits of the hyperborean hunters with whom they were occasionally brought into contact. The Eskimo, however, is neither ignor-

ant of the use of fire, so indispensable to him in his rigorous climate; nor is he an exception to the fitting definition of man as "the Cooking Animal;" though in his peculiar condition of exposure to an arctic winter, raw blubber is at once a necessity and a luxury.

In one respect, as already indicated, the Eskimo occupy a peculiar position on this continent. They are the only race common to the Old and the New World; and, if we accept the conclusion arrived at by the author of "Early Man in Britain," they constituted an Old World race to all appearance before this New World had come into existence. The cave men of Europe's palæolithic era, the contemporaries of the mammoth, and other long-extinct mammals of central Europe, have naturally excited an unwonted interest, as their arts and their remains have been brought to light in recent years. A people of lowest type, as illustrated by the famous Neanderthal skull, that of the Forbes quarry near Gibraltar, and of the Gourdon grotto, with some imperfect traces of others, all classed under the common term of "The Canstadt race," is now assumed to represent the earliest, if not indeed the primæval man of ancient Europe. So far as rudest flint implements afford any evidence of his condition, we might class him with the Bosjesman, the Australian, or the Patagonian of our own day. The evidence, however, in proof of the existence of this Canstadt savage race of palæolithic Europe, rests as yet on insufficient grounds. Curiously, indeed, Professor De Quatrefages has drawn attention to the fact that not only are heads of the Neanderthal type to be met with in modern Europe, in some examples pertaining to men of exceptional intelligence; but the skull of Saint Mansuy, Bishop of Toul, of the fourth century, surpasses the Neanderthal cranium in some of its most simian features; and that of the sagacious and politic hero of Scottish independence, Robert the Bruce, "is a reproduction of the Canstadt type."

But however uncertain our conclusions may as yet be relative to this assumed primæval European type, there is no doubt as to the Cro-magnon race of the reindeer period of southern France. Examples have, indeed, by no means been confined to that area. The Enghis skull was found, with other human remains, embedded in a breccia along with teeth of the fossil mammoth, rhinoceros, horse, and reindeer, in a cavern on the left bank of the Meuse; and the Mentone cave, to the south of the Alps, disclosed an undisturbed sepulchre of the same ancient hunter race. But a

special interest attaches to the remains brought to light in 1858, in the rock shelter of Cro-magnon, in the valley of the Véserè. Three men, a woman and a child, had all been buried in the cave. From their remains it is seen that the race was unusually tall, and bore equally little resemblance to the Neanderthal or "Canstadt" type, or to the modern Eskimo. The best preserved skulls—those of an old man and a woman,—are finely proportioned, with large, high foreheads, and great cerebral capacity. M. Broca stated that of the man to be fully 1590 cubic centimetres, or 96.99 cubic inches; and Dr. Pruner-Bey says of two of the male skulls and that of the female, they "have a cranial capacity much superior to the average of the present day." It may remind us of Mr. Alfred Russel Wallace's remark that "natural selection could only have endowed savage man with a brain a little superior to that of an ape, whereas he actually possesses one very little inferior to that of a philosopher."

Whatever differences of opinion affect the determination of the probable age of the Cro-magnon race, they unquestionably pertain to a period so remote that the very earliest historical traces of man in southern France scarcely seem to bring us any nearer to the period which they represent. Their physical characteristics have, therefore, a special significance. The skulls are dolichocephalic, with the frontal bone high and well arched, a graceful fronto-occipital curve, and well-balanced symmetrical proportion throughout. The profile of the old man indicates an expressive contour, the face long, the nose very prominent, and the frontal sinuses but slightly developed for a male. The full face presents a well-proportioned oval, with pointed chin. The one feature detracting from its otherwise attractive expression would seem to have been the unique character of the long and narrow eyes, as indicated by the unusual form of the orbits. At the same time it is to be noted that this well proportioned head bears ample evidence of the exposed life of the wild hunter. The features are rugged, as of one subject, through a long life, to all the hardships of a rigorous climate; and numerous strongly marked impressions of muscular insertions accord with the conditions of savage life.

This is the type of an altogether remarkable prehistoric people, the artistic race of the palæolithic era, to whose skill we owe the contemporary etchings and carvings of the mammoth, the fossil horse, the reindeer, and other mammals of that strangely remote

era, when the conditions of life in southern France most nearly resembled those of Rupert's Land or Labrador at the present day. Some of their artistic efforts embrace vegetable as well as animal life; and in their graphic outlines, there is a freedom of handling, an eye for perspective, and even what may truly be called an inventive skill, altogether remarkable in a people ignorant of metallurgy, and living in the condition of rudest hunter life.

Such are the characteristics of the cave men recovered from the rock-shelter of Cro-magnon, in the valley of the Vésère, lying above the long accumulated debris which proved the palæolithic era of its occupants. These ancient hunters of the Garonne and the Pyrenees, the contemporaries of the mammoth and other extinct mammals, and of the grizly bear, musk-sheep, reindeer, and other species now existing only in extreme northern latitudes, had occupied the cave throughout a prolonged period. The charcoal deposits prove the kindling of their fires through long series of years; with recurring intervals of considerable duration. Intermingled with, and overlying, the beds of charcoal, are flint implements, broken bones, and the like debris of a savage hunter's dwelling: in this respect presenting considerable resemblance to the site of an Eskimo settlement. Those accumulations went on until the cave was filled up so nearly to the roof that the hunter could no longer even crawl into it for shelter; and then it was devoted to its final use as a place of sepulture. Had the human remains recovered under such circumstances been characterized by extreme development of the superciliary ridges, a low, narrow, retreating forehead, and other brutish characteristics assigned to the "Canstadt race," there would probably have been no questioning the assumption that in them we have the type of the earlier Palæolithic occupants of the cave: the artistic hunters, to whose skill we owe the spirited life-sketch of the mammoth found in the neighboring La-Madelaine Cave, engraved on a plate of mammoth ivory. But the hypothesis adopted by Professor Boyd Dawkins that the clew to the ethnology of the palæolithic race of the Caves is recovered by the identification of the ancient tool-makers with the Eskimo, could only be maintained by assigning the human remains to a later age than the underlying debris. He accordingly rejects the opinion, so consistent with the general evidence which has satisfied M. M. Lartet, Hamy, De Quatrefages, and other equally qualified judges, that in the old man of Cro-magnon we

have the type of the palæolithic race contemporary with the mammoth: the artistic sculptors and draftsmen of that remote European era.

In discussing the fascinating idea which would recover, in the hyperboreans of our own northern frontiers, the men of the same migratory race that, before the close of the pleistocene age, followed the musk-sheep and the reindeer into their northern haunts, Professor Dawkins reviews the manners and habits of the Eskimo, a race of hunters, fishers, and fowlers, accumulating round their dwellings vast refuse heaps similar to those of the cave-men of ancient Europe. The implements and weapons of both do indeed prove that their manner of life was the same: and as he notes the use at times by the Eskimo of fossil mammoth ivory for the handles of their stone scrapers, he adds: "it is very possible that this habit of the Eskimos may have been handed down from the late pleistocene times." But what strikes him as "the most astonishing bond of union between the cave-men and the Eskimos is the art of representing animals:" and after noting those familiar to both, along with the correspondence in their weapons, and habits as hunters, he says: "all these points of connection between the cave-men and the Eskimos can, in my opinion, be explained only on the hypothesis that they belong to the same race."

The hypothesis is a bold one which would thus assign to the rude arctic hunters of this continent a pedigree and lineage compared with which that of the Pharaohs is but of yesterday. To the geologist who fully realizes all that is implied in the slow retreat of the palæolithic race of the valley of the Vézère over submerging continents since ingulfed in the Atlantic, and through changing glacial and subglacial ages, to their latest home on the verge of the pole, the time may suffice for any amount of change in the physical characteristics of the race. But if these have vanished how is the lineal descendant of the palæolithic cave-men to be identified? Not by mere imitative art: for that is common to many widely dissimilar races of the American continent. Professor Dawkins says truly of the cave-man, "he possessed a singular talent for representing the animals he hunted: and his sketches reveal to us that he had a capacity for seeing the beauty and grace of natural form not much inferior to that which is the result of long-continued civilization in ourselves, and very much higher than that of his successors in Europe in the Neolithic age. The hunter

who was both artist and sculptor, who reproduced with his imperfect means at one time foliage, at another the quiet repose of a reindeer feeding, has left behind the proof of a decided advance in culture, such as might be expected to result from the long continuance of man on the earth in the hunter state of civilization." All this is correct in reference to the art of the Cro-magnon carvers and engravers, and seems in full accordance with the fine heads and great cerebral development of the ancient race; but it would be gross exaggeration if applied to such conventional art as the Eskimo arrow-straightener which Professor Dawkins figures, with its formal row of reindeer and their grotesque accessories. The same criticism is equally applicable to numerous other specimens of Eskimo art, and to similar Innuit, or western Eskimo representations of hunting scenes, such as those figured by our associate Mr. William H. Dall, in his "Alaska," which he describes as "drawings analogous to those discovered in France in the caves of Dordogne."

The imitative faculty and artistic skill of the old Mound-builder race are very familiar to us; and have furnished valuable evidence of a knowledge by them of a tropical fauna, including animals of the southern continent, suggestive of the probable direction of their own migrations, and their consequent affinity to southern races. Within our own Canadian Dominion the arts of the Queen Charlotte islanders are no less worthy of note. Their curiously conventional style is shown alike in their idols, or manitous, elaborately carved in black argillaceous stone, and in the corresponding decorations of their lodges. In front of each Haida dwelling stands an ornamented column formed in many cases of the trunk of a tree large enough to admit of the doorway being cut through its substance. This column, or obelisk, is carved throughout its whole length in their peculiar conventional style of ornamentation, suggestive at times of affinities to Peruvian sculpture; or, again, of borrowed art of possible Japanese origin. But already the imitative faculty of the Haida artist leads him to revert to European models; his traditional patterns and devices will speedily be among the lost arts of this continent, and the race itself, it is to be feared, is doomed to speedy extinction. All the more urgent is it that no time shall be lost in the accumulation of every available fact, and illustrative specimen of their curious art. Already a valuable contribution to this has been furnished in

Dr. George M. Dawson's "Report on the Queen Charlotte Islands," published as one of the Reports of the Geological Survey of Canada.

The Tawatin Indians on the Fraser River work with no less ingenious skill, and in a like style of combined imitative and conventional art, suggestive at times of curious analogies to some of the finished sculptures of Yucatan. Some of their ivory carvings are executed with a minute delicacy of workmanship such as no Eskimo carver could surpass; but with the same kind of conventional ornamentation as is in use by the Haida artists, strongly suggestive of inherited modes of thought, and traces of intercourse or relationship with the ancient civilized races of Central America.

There is thus no need to assume for the imitative arts of the New World a European source in the remote dawn of pleistocene times. Nor is the identity discernible between certain harpoons and other implements of the ancient hunters of Central Europe and those of the arctic Americans of our own day much more demonstrative of derived arts or community of race. Within the comparatively narrow range of needful weapons or implements, the correspondence notable between some of those of the palæolithic cave-men and of the Eskimo amounts to little more than what is seen in flint arrowheads, stone hammers, and the more common primitive tools of all kinds, executed under nearly similar conditions of life. "The absence of pottery" proves little more than the absence of tropical vegetation; for both were nearly equally impossible under the conditions of climate. The preference for bone and ivory as the materials for their arts is equally due to climatic conditions which render rock and flint generally inaccessible throughout the greater part of the year. The points of agreement are, in truth, little more than are to be anticipated among savage tribes living under similar conditions of climate.

If, however, the skulls of the Cro-magnon cave-men resembled those of the Eskimo, or the underlying debris revealed any traces of crania of the Eskimo type, there would then be good reason for giving consideration to the bearing of any supplementary evidence depending on correspondence in arts, usages and habits. But neither the Cro-magnon cave, nor any other of the caverns of the district, otherwise so rich in archæological and palæontological traces, has yielded the needful evidence. The contrast between the large, well developed Cro-magnon race and the stunted,

almost dwarfish Eskimo at once attracts attention. But much greater difference in stature would find ready solution in the privations of an arctic habitat prolonged through unnumbered ages. The notable fact, however, is the absolute contrast in every respect. The Eskimo physiognomy is of a poor Mongolian type. The nose is flat, and the cheek bones are very prominent; the tendency in the skull is towards an acrocephalic form, narrow and long, with the parietal bones frequently meeting at an angle at the sagittal suture. The one possible point of resemblance that could be suggested with any acceptance would be the eye, which in the Eskimo seems often narrow and oblique. This, however, may be apparent only, traceable to the habits of a people one-half of whose year is an unbroken midnight; and who grope in the darkness of their obscurely lighted snowhuts. Certain it is that the long, narrow orbits of the Cro-magnon skulls are not represented in the modern crania.

Sufficiently extensive opportunities of studying the Eskimo cranium have come within my reach to afford me some fair means of forming an idea of the predominant type. In 1862, through the kind services of the late Dr. J. Aitken Meigs, I enjoyed the advantage of carefully examining a series of one hundred and twenty-five skulls, obtained by Dr. Hayes during his Arctic explorations, and making drawings of some of the most marked examples. I have also examined and taken careful measurements of other examples including Western Eskimo, Innuit, and Tschuktchi crania, in the collections at Washington. With the resulting impressions in mind, it is impossible to look on casts of the large and finely developed Cro-magnon skulls now in my possession without being struck with the extreme contrast between them and the Eskimo crania. No wonder that they prove a stumbling block to evolutionists, who look for something of a totally opposite character in the Troglodytes of the palæolithic, or pleistocene age. M. M. Lartet, Hamy, De Quatrefages, the editors of the *Reliquiæ Aquitanicæ*, and other equally competent authorities, have had no difficulty in accepting the evidence that the reindeer hunters of the Vésère lay there intombed in the cave which had so long been a shelter to men of the same race. Had the Neanderthal skull been found under similar circumstances, no doubt, founded on its lower cerebral capacity, would have interfered to prevent its recognition as the type of the artist race to which we owe the life-picture of the mammoth. But Professor Dawkins not only notes that the human

remains were deposited in an abandoned palæolithic cave, when it had been nearly filled up with the accumulated debris of successive occupants; but he assigns the remains to the later Neolithic age, notwithstanding the absence of any accompanying relics of the art of the polished stone period.

But I have already exceeded the reasonable limits of an address to this Section of Anthropology, and must leave unnoticed various further points in reference to the aborigines of the Dominion, illustrative alike of the physical characteristics of our native Canadian tribes, and of some special points of significance in relation to their arts. One deduction, however, may be worthy of future consideration. If it be a fact borne out by much independent evidence, that from the extremest northern range of the arctic Eskimo, southward to the Great Lakes, and beyond this, especially to the east of the Alleghany Mountains, amid considerable diversity of ethnical characteristics, the dolichocephalic type of head prevailed; whereas among more southern tribes, such as the Osages, Ottoes, Missouris, Dacotas, Cherokees, Seminoles, Creeks, and many others, including the Florida Indians, the short, rounded, or brachycephalic head appears to have been universal: this seems to point to a convergence of two distinct ethnical lines of migration from opposite centres. In this, as I believe, the evidence thus derived from physical characteristics confirms what is indicated by wholly independent evidence of language, traditional customs, and native arts.

PAPERS READ.

Notes on Mount Atlas and its Traditions. By R. G. Halfburton, of Ottawa, Canada.

[ABSTRACT.]

In Dec., 1881, to while away the time at Tangier, where continuous east winds for weeks kept invalids within doors, I turned my attention to the traditions and folklore of the natives of Marocco. The field was represented as a barren one. I was told that the country was inhabited mainly by the Berbers (or Barbars, whence the name Barbary); that, though composed of different tribes, they spoke substantially the same language; and that having adopted the religion of Islam, they had forgotten their old traditions and superstitions. The Rifflans, known still in history as the Riff pirates, inhabit the northern portions of Mount Atlas. To the south of that mountain is a tribe of excellent artificers in brass and copper, called Shelluhs, Shilhas or Silhachs, who inhabit the province of Sūs, and are therefore called Susis. There are other tribes still farther south.

It soon leaked out that the faith of Islam sits very lightly on some of the Susis, and that many of them really have no religion, or have some ancient superstitions which they preserve in secret. Whether they really believe the myths and traditions which they repeated to me, or merely regard them as our peasants do their fairy tales and folklore, it is hard to decide. One of them told me he was not sure that he was not as much of a Christian as of a Mohammedan — a statement that if overheard by the Moors might have cost him his life.

I made careful inquiries from several Rifflans, a Maltese who had travelled in the interior of Africa, disguised as a Moor, the town time-keeper or astronomer at Tangier, some Jews of Casablanca and Ophran, two Susis at Tangier, and one at Mogador, nearly all of whom, though illiterate, and unable to read Arabic, were learned in oral tradition and Berber folklore.

The results of my inquiries proved that there is a marvellous collection of ancient myths, legends, etc., among the Susis, which carry us in succession to Britain, Greece and Rome, Phœnicia and Egypt, and even to Babylon, while one very remarkable festival seems as if it had reached them from the Aztecs.

The Great Mother of the Greeks, Damater, appeared as *Ta Mata*, "the mother, who presides over the corn fields." Apălo, "a good god who comes and plays on a harp," suggests the inquiry, is not Apălo the original form of the name of the god Apollo?

At certain intervals, as the Greeks believed, Apollo used to desert his shrines, and go far off to the blessed Hyperboreans, with whom he danced and sang until the rising of the Pleiades.

The ancient Greeks themselves seem to have regarded the Atlas country as the favorite abode of Apollo. Atlas was a Hyperborean, and the western Ethiopians, who inhabited that country, were also Hyperboreans, a semi-divine race, from whom the Greeks derived many of their most ancient rites. It is supposed that they were called Hyperboreans from their enjoying a climate where the cold north wind was unknown, but the same name would be even more applicable to the people of the interior of Africa. I am inclined to think that it meant "those who dwell beyond Borium," a very ancient town on the borders of Mauritania, to which I shall hereafter refer.

A Susi described to me a staff ornamented with ribbons, which is called a *thurosis!* The names too of Mata, Kĕra and Zerēa, recall that of Ceres. The "Great Mother's" image is bathed at the end of her festival, as it was in the holy island of the Germans, at the river Almon, and in Athens at a feast of Minerva, which was therefore called *Plynteria*. We are even reminded of a similar rite at the end of the feast of the god of agriculture of the Fijians.

The god Adon is still believed to have been slain by a boar, and heaven and earth all weep for him. "He was greatly beloved by Tachal and Isai." It seems that some *festive dirges*, like the Maneros of the Egyptians, and the Linus of the Greeks, which were sung at their banquets, can still be traced in the *Accasili Maneros* and the *Wălĭnas* of the Susis. Diodorus Siculus tells us that in the Atlas country a divine youth, Hesperus, went up at night to the summit of a mountain to study the stars, and a great wind carried him away. To this day (I am told by the Susis) "the women go up on the mountains with music, weeping, in their search

for Walinas. He was the brother of Panis, an old god who invented pipes called Kraf or Kalifer, and who was also called Itada."

One of the Susis asked me if I would like to hear "the story of the man who wished to steal the cows," and upon being asked to proceed, retailed a familiar bit of classical mythology: "There is a great mountain in the sea, where there were 300 cows the property of Geryon, and when the sun set they used to appear. It was a very rich place, and a navigator, who wished to steal the cows, sailed thither, and entered a great cave, from which he never came out; and his name was Herakles."

There can be no question that this mountain, the true Mount Atlas, was the Peak of Teneriffe, in which there is a vast cave that has never been fully explored. The Atlas country was the scene of the labors of Hercules, and of the feats of Perseus, who turned Atlas into a mountain by showing him the Gorgon's head.

Herodotus says that the dress of the statue of Minerva was borrowed from the Atlas country, where clothes of kid-skin are made and colored with great skill. Such dresses may still be seen in the Museum of Las Palmas, taken from the mummy caves of the Guanches. He also suggests that the story of the head of Medusa being encircled with snakes, arose from the head having been placed on a shield ornamented by the Atlantes with a fringe consisting of long strips of leather, which at a distance might well look like snakes. These fringes are still used by the Susis.

Even the Greeks admitted that one of their most unintelligible myths, that of the fifty daughters of Danaus, who were doomed in Hell to the task of filling sieves with water, came from Africa, and they therefore gave the brother of the Danaides the name of Ægyptus, though modern Egyptologists have failed to meet with the myth in the religion of the ancient Egyptians. Many years ago a devout believer in "Arkite lore" detected an allusion to the deluge in the name of Danaus, which he traced to *dan*, "water" or "rain." If he had called the fable a "rain myth," he would perhaps have been nearer the mark. It was evidently carried by the natives of the Atlas to Greece, where in time its original meaning was forgotten, for we still find it in the folklore of the Susis, one of whom told me that "there is an old King in the stars of rain, who has many dancing women, who hold sieves filled with water; and when he wishes them to dance, he thunders. The louder grow the peals, the quicker and wilder grows the dance,

during which the sieves are emptied, and the water falls to the earth in a thunder shower."

These are a few only of the traditions and beliefs that carry us to Greece and Rome. We meet with Phœnician traditions also as to "Isiri who taught the three letters;" while the belief in an imperfect creation, in which the forms of animals and men were blended together, recalls a similar tradition of the old Chaldæans. Of Egyptian ideas there are perhaps traces in a belief as to seven brothers who sail in their ship across the sky, and carry with them the spirits of the dead.

The Susis have a May day festival at which the "pole of Maia" is set up, at the summit of which is a doll composed of heads of wheat. Saints climb up the tree and scatter the wheat among the people, calling it "our life," "our sustenance." The Mexicans used to erect an enormous cross, the symbol of rain, and on its summit was placed a similar doll, which when reached by those climbing the pole, was scattered among the people, who treasured the fragments as something sacred, while the deity represented was called "our life," "our support." The coincidence is certainly very remarkable, for precisely the same words were addressed by the Iroquois to the three beneficent maidens residing in the Pleiades, who brought each her gift to mortals, the maize, the squash, and the bean.

Herodotus has mentioned that the peculiar cry of the women at the rites of Minerva, called *ololuzein*, was borrowed from the Libyan women, "who sing it very sweetly." I have heard this peculiar chorus or cry which consists of a quick repetition of the word ălō,— ălō ălō ălō ălō. I am told that it is raised at the end of the feast of Mata when water is poured over the image of the goddess; and am reminded of the shout that resounded throughout Fiji at the close of the feast of Ratimaimbulu, who no doubt was the same as the god Alo-ălo of the adjacent Friendly or Tonga Islands. It would be exceedingly interesting if it should prove that the cry raised and carried from town to town in Fiji was, like that at the feast of Mata, *ălo ălo ălo ălo*.

If there is any foundation for the belief of the earliest nations and of the Susis themselves, that that country was once the seat of an ancient civilization, how can we account for the early rise of a great commercial and maritime people near Mount Atlas? The answer may be given in one word—*gold*. One of the natives examined at Mogador was Mordecai Rhibo, a Jew from Ophran, which he de-

scribed as a very ancient town, which from remote ages has been the entrepôt of the Timbuctoo gold trade. At that point the caravans separate and go in different directions, one to the city of Marocco, and the other beyond Tripoli.

There are Jewish tombstones there dating back nearly to the beginning of our era. There is also a vague tradition that there is somewhere in the interior a tribe of Jews who are warlike and independent, and who have no knowledge of the Second Temple.

I ventured to suggest, when I read my paper, that we have good reason to believe that the *Ophir* of the Bible, and *Saba* or *Sheba* may yet be traced to that part of Africa. This has since been confirmed by my finding in Procopius, that one of the two great divisions of Mauritania was called *Zaba*. He also states that there was in his day (*circ.* A. D. 550) a very ancient city in Numidia, on the borders of Mauritania, called *Borium*, which from the most remote times had been inhabited by Jews, who had never paid tribute to any one. There was there too a very remarkable Jewish temple, which the Jews believed had been built by King Solomon. As the adjoining country was called *Zaba*, and as no trace of the first temple has ever been found in Jerusalem, it is not impossible that this was the temple to which the Queen of Sheba paid her famous visit.

It is probable that nomad Berbers, known as Sabæans, had, in the days of Abraham, as they still have, the monopoly of the gold and ivory trade of central Africa, and with their caravans carried its products, including slaves, parrots and incense, to central Asia. The Sabæans were, like the Susis, astrologers, necromancers, traders and robbers, believing in the seven heavens, and worshipping the seven stars. It has been conjectured that in the Puranas, traditions of an earthly paradise, differing from that of the general Hindoo system, seem to point to Africa (see Smith's Dictionary of Bible, *tit.* "Eden"). But the place to which we must turn cannot be, as Smith's article suggests, in southern Africa, but rather in northwestern Africa, where the garden of the Hesperides and the Islands of the Blessed were situated.

The Susis have a belief in seven heavens called *Saba Samagwats*. *Saba* means seven; the other word, by the aid of a pre-Malayan language in Malacca, can be interpreted, as it is the same as *samangats*, "the spirits of the Blessed," who reside in Pulo Bua, "the fruitful island in the west." I have also been shown

a diagram of the mystic ladder for the descent and ascent of souls, called Jacob's ladder, by the poet of the Bible. It is like the golden candlestick with seven lamps that symbolized the seven virtues or gifts of heaven in the days of Moses. These Sisters are necromancers and sorceresses, resembling the witches in Macbeth and Lear. One of them is the oldest offered to bring up any spirit that I might wish to see in Hades,—an offer that reminded the question of the Witch of Endor, "And the woman said, whom shall I bring up unto thee?"

To show how to construct the peculiar traditions of these people are, I may mention that a Sister and the giant of a mountain are the same. In the [illegible] we have the form of the word somewhere between Is and the Sphinx, which have the face of boys. Herodotus, 2,500 years ago, was told precisely the same story, and found a wide belief among the Egyptians that he traced to their mythology.

It may seem premature to endeavor to account for this strange assortment of myths from so many parts of the world. A few facts connected with the history of Mount Atlas may be suggestive of answers to all this.

The earliest [illegible] of [illegible] Mount Atlas are to the great [illegible] the Hesperides, as it was to the Greeks of that epoch. The Sisters [illegible] the Atlantes [illegible] the world. Diodorus [illegible] that the Atlantes [illegible] the most ancient [illegible] and that their [illegible] was [illegible] [illegible] [illegible]

navigator, "knew all the depths of the ocean," and who taught Hercules astronomy, was also called "*a shepherd.*" The Susis (at least the nomad portion of them) are still called by the Arabs *Beni Baccar*, "the sons of the cow," the pastoral people. Even the word "Sos," according to Herodotus, meant, in the sacred language of Egypt, "cattle."

It has been conjectured that the light-haired race, that from the most remote ages occupied the Atlas country, lost their language and adopted the Berber tongue. I have recently found not less than seventy familiar English words in use in Marocco, and it is probable that careful inquiries will show a still larger survival of them in that country.

That Tarshish was a port in the Atlas country seems exceedingly probable. It is admitted that it was situated either in Spain or somewhere on the Atlantic seaboard. It was apparently the Birmingham of early ages, for its brazen dishes were an extensive article of export even to Phœnicia. We have now no trace of such an industry in Spain, but we have the clearest evidence that from the most remote times it flourished in Sus, the people of which seem to be a survival from the Bronze Age, for their principal trade is the manufacture of brass dishes, which they chase with marvellous taste, and which are perhaps known to us as the "beaten dishes" used in the Temple of Solomon. Leo Africanus says that Ifran was in his time (A. D., 1550) a seat of this industry, from the existence of extensive copper mines near that place. Though it is only known throughout Marocco as *Ephran*, its inhabitants call it *Ophran*, and such is the name given to it in the latest English map of that country.

To the ancient Jews it was probably known as Ophir, that mysterious city which tradition says was the capital of the Sabæans, and connected with Tarshish. That the latter was a port of Ophir or Ophran, and situated on the coast of Sus, seems probable from some curious incidents in the history of Jonah, who instead of going eastward from Jerusalem to Nineveh, went to the remotest west, in a ship bound to Tarshish.

To my surprise one day, a Susi told me that a great prophet was swallowed by a large fish and cast up by it on the coast of Sus, and I at first assumed that he had picked up this story from some Jew; but I have since discovered that it is an ancient local tradition as to Hercules, the hero of the Atlas, who must have sailed

from a port in western Marocco, for on ancient maps we find a harbor there called "the port of Hercules." The two stories (whichever may have been the original one) point to a tradition connected with the Atlantic and the coast of Sus.

Many myths have a local origin in some natural phenomenon that primitive races cannot explain, except by the supernatural. We may find the key to this venerable tradition of antiquity as to Hercules in the existence, near the place in question, of sharp pointed rocks, which are fatal to whales that may be driven on them by a storm. Hercules, we are told (see Took's Pantheon, Part II, ch. 1; Ovid Met., II) "delivered Hesione, the daughter of Laomedon, King of Troy, from the whale in this manner; he raised, on a sudden, a bank in the place where Hesione was to be devoured, and stood armed before it; and when the whale came seeking his prey, Hercules leaped into his mouth, slided down his throat, destroyed him, and came away safe."

The tradition, which I had heard from a Susi, also existed in the days of Leo Africanus, who tells us that in the town of Messa, the name of which means "Lord," and at which the natives believe that the promised Messiah will appear, is a very ancient and sacred temple, the rafters of which all consist of the bones of whales, in commemoration of Jonah having been cast up by a whale on the adjacent seashore; and that in confirmation of this belief, the Moors pointed to the fact that all the whales immediately die that pass to the right of the temple. The historian was inclined to think that there might be something in the story, from his seeing at the time a dead whale floating near; but a Jew ridiculed the superstition of the Moors, and explained the origin of it, by the existence and effects of the reef in question,—that bank, no doubt, that Hercules raised suddenly for the destruction of the whale. I think there can be little question that this ancient whalebone temple of Jonah was originally built in honor of Hercules, the hero of the Atlas country; and it is possible that the story connected with it may have been carried back to Palestine by the Jews of Ophir, and have been preserved by them in the history of the prophet Jonah.

May not they also have brought back from Ophir the Book of Job? That work is a singularly faithful picture of the productions, animals, traditions, manners and astronomical ideas that are still to be found south of Mount Atlas. There to this day wan-

dering bands from the desert sweep down upon the herdsman and the shepherd, and rob them of their herds and flocks; and the ostrich, the hippopotamus, the monsters of the ocean, the birds, the beasts, the treasures of the mine, and the stars, that are described by the patriarch, are still familiar to its inhabitants. Even that auspicious constellation whose "sweet influences" are celebrated by him, is known by the same name to the Susis, who call it *Kimah* ("a furrow"), or "the stars of tillage" as it is termed by the far distant Bechuanas of south Africa (*Silemela*).

The people of Sus also believe that there is a certain night in the year when the stars hold a solemn festival, in which all the angels and the spirits of the great kings of old take part. The very words of the song of the Pleiades, who are known in the New World as well as in the Old, as "the dancers," "the Celestial chorus" of the Greeks, "the Heavenly Host" of the Hebrews, and "the seven dancers" of the North American Indians, are familiar to ears that can catch "the music of the spheres," and have been repeated to me by one of those favored mortals, a Susi wanderer from the Sahara:

> "Oh Moon, oh Mother, we hold our feast to-night,
> We are dancing before God, between heaven and earth,"

words that recall Milton's allusion to those "morning stars that sang together with joy" at the creation,

> "And the Pleiades before him danced,
> Shedding sweet influences."

This celestial festival evidently takes place on that night in November, when the full moon and the Pleiades are on the meridian together, for there is a Susi love song,

> "Oh come to me my love, and long remain,
> For the Pleiades are meeting the moon to-night."

On that very night in November some tribes of the Australians still celebrate "the sweet influences of the Pleiades," and hold a grand corroboree in their honor, for "they are the children of the Sun and Moon," and "are very good to the blackfellows."

Even the early Egyptians seem to have borrowed many of their religious ideas from an older civilization in the Atlas country, for it has been conjectured that all the magical features of the ritual

of the Egyptians, and their belief as to the dangers attending the passage of the soul to Hades, were derived from the people south of the Atlas (see Smith's Dict. of the Bible, *tit.*, "magic").

It is surprising to find that a country, venerated in the days of Homer, as peopled by "the just Ethiopians" who were nearer to the gods than other men, and at whose banquets even Jupiter was sometimes a guest, a country, too, associated with paradise and the abodes of the blessed, should, a few hundred years after his time, have been lost sight of by the world. Herodotus does not refer to its past history, and learned little of the country south of the Atlas. Strabo says that in his time it was a *terra incognita*, for armies and even travellers had seldom reached it, and the few natives that visited Greece either invented fables about it, or were unwilling to tell what they knew about it. To this day it is closed against Europeans, none being able to visit it except by the hazardous experiment of passing for a Moor.

Leo Africanus, however, himself a Moor, who has described that country as it appeared in his day, and has told us how the Arabs had ravaged it, destroying the cities, and burning the ancient books of the Berbers, states that near the walls of one town, the stones of which, as large as those employed in the construction of the Coliseum at Rome, had defied the fury of the invaders, gold and silver medals are to be found, with characters which he had in vain endeavored to decipher; and that everything indicates that at a former period these cities must have been the homes of a prosperous people.

This paper is the first attempt that has been made to draw attention to the traditions of a country that must once have played an important rôle in history. It is possible that future and more careful inquiries as to it may throw much light on the commerce, and perhaps on the origin of the Jews, and on many obscure points connected with early civilizations and mythologies; and that they may even justify the belief of the Susis that they are descended from the most ancient nation in the world, and the boast of the old Atlantes, that their land was "the birthplace of all the Gods of antiquity."

I intend, if my health permits me, to revisit that country, and to follow up these researches.

SOME HITHERTO UNNOTICED AFFINITIES BETWEEN ANCIENT CUSTOMS IN AMERICA AND THE OTHER CONTINENTS. By J. H. PHENÉ, of London, England.

[ABSTRACT.]

THE author of this paper explained that as one of the great problems to be worked out, or which at least it is desired should be worked out by all who have given any attention to the subject, is that of the races and civilizations which once flourished at the southern end of North America, and which had evident connections with the more northern parts of what are now the United States, through those great ducts, the Mississippi and other vast river valleys, tending to the latter direction.

The customs which are shown to have existed in the great river valleys of America, though read with difficulty by the light of the strange monuments still existing there, seemed to the author to have had parallel existences on the other continents. In evidence of this he gave illustrations, by drawings and diagrams, of many earthworks and stone constructions which had been examined by him, and which agreed in the method of executing the earthworks, of arranging the plans and designs, and in the evidently similar purposes for which they were designed, and to which they had been devoted.

The points on which he rested his argument for the affinity of the American mounds and earthworks, and, necessarily, the customs with which they were connected, with those of Great Britain and a large number of similar works in France, Spain, Greece, Asia Minor, Persia and China, were, in the first instance, the existence of relationship to each other, always in the vicinity of rivers, of mounds representing animal forms, and—with some special exceptions—the close proximity of vast works of camps for defence, huge enclosures or oppida for civil occupation, and other evidently sacred enclosures for solemn rites, worship and sepulture. In America these evidences went no farther, but in the vicinity of the mounds he had traced in Western and Eastern Europe, and in Asia Minor, and still farther East, not only were all these features attendant but the localities also abounded with mythological and traditionary legends, and the retention of strange and weird ceremonies to the present day.

He selected as illustrations of the American mounds, those in the forms of serpents, the alligator (or mythical dragon), and the

human form. The serpent was shown by Mr. Squire to have been executed in two ways, viz.:—by the solid continuous serpentine form, and by a series of symmetrical mounds uniformly placed in curves. The reader gave examples of each of these, on diagrams, and explained that they existed in large numbers in Great Britain, similar in proportions and construction to those of the American mounds, which were kindred in form, and accompanying these were, he stated, in every case, extensive areas occupied by similar camps and enclosures for civil occupation, or oppida, and also separate enclosures for worship and sepulture. The characteristic physical, natural and art features were also curiously persistent in each case, a triple imagery having been in very many instances studiously represented, both by places of selection, as the vicinity of triple peaked mountains, and in construction, by triangular chambers and triangular enclosures in or about the head of the animal forms, as well in the American as in other similar mounds. Since his arrival at Montreal he had heard of indications of this having been an Indian burying ground. They appeared often to have selected places of previous occupation, as in the mounds; and the triple hill and trianglar piece of land at its base were significant. The works in Great Britain, Spain, France, and other countries, were found to contain chambers filled with cremative matter, and had studied arrangements for preserving the outlines of the animal forms, as shown in the diagrams, and were generally surrounded with vast lithic arrangements, and each, as a rule, was in the centre of an extensive necropolis of the primitive inhabitants. The legends and traditions clustering about these places were always of the same class in the other continents, and almost always had reference to man as the possessor and the serpent or dragon as the persecutor or destroyer, and this whether the tradition was one of uncultured and primitive existence, or of the highest classical art ages and localities; and though such traditions did not exist in America, yet not only did their huge mounds simulate these forms, but there had been dug up from their mounds rude sculptured figures of the human form and also of entwined or coiled serpents, showing that in their construction the same ideas, if not similar legends and traditions, had existed, as those which originated the gigantomaxia of Pergamos.

Amongst other numerous and striking examples of these forms in Great Britain, corresponding with the animal and human forms

in America, were some very remarkable and yet but little-known representations of the human form belonging to a rude and unlettered age, perhaps corresponding to that of the semi-barbarous age of Mexico, which are found on the coasts of Devonshire and South Wales sculptured in stone, a very remarkable example of which also existed near West Hoathly, in Sussex, which was traditionally stated to have been worshipped as a potent deity (the goddess Andras) by the early Keltic inhabitants. In the vicinity of these rude figures in the western and eastern extremities of the South of England, were two enormous *intaglio* representations of the human form, corresponding to the *intaglio* forms in Wisconsin, only one of which had been mentioned, and that in a very cursory manner, by writers on the comparison of these works with those of America, while their surroundings had been completely overlooked. The one not referred to was by far the more important, both in size and similarity, being in the exact attitude of the mounds in the human form of the American mound-builders. About midway between these two figures was an equally enormous and well-known representation by *intaglio* of a horse. These figures were all cut in the chalk hills, and might be said to be protecting deities of the three localities in England most abounding in remarkable and vast areas of camps, oppida or settlements, and places of worship. The whole district from Dorsetshire to Land's End being commanded by the gigantic form in Dorsetshire; that of the miles of stone avenues on both sides of the Medway in Kent, terminated by Kit's Coity House at one end, several *alleés couverts* at the other, with the huge stone figure at West Hoathly and the wood of Anderida to the south, were commanded by the similar gigantic *intaglio* figure in Sussex; and the central and more important districts of Stonehenge, with the primitive and stupendous temple of Avebury, its sinuous avenues of stone, and extensive earth embankments, the great tumulus of Silbury Hill, the avenues of stones near Amesbury, in the style of those at Carnac in Brittany, the large number of camps, the trilithon called the "Devil's Den," and the great dolmen known as "Weyland Smith's Cave," appear under the protection of the "White Horse." Metallurgy, a great feature with the mound builders of the Mississippi, was also the peculiar occupation of one, if not of two, of these three districts of southern England and of many of the other localities in which similar works were found in Europe and Asia. And such metallurgy was for all utilitarian purposes con-

fined in every such country in which these works exist to the two metals—tin and copper—and analysis showed the proportions in the amalgamation of those metals to have approximated in America, Europe and Asia. The parallelism went further and was continuous in its tract eastward. Brittany, Spain and France on each side of the Pyrenees, and so on to Asia Minor, had the same combined features. In the valley of the Meander in Lydia, the vast figure of "Niobe," mentioned by Homer, still existed, sculptured on the side of Mount Tmolus, and, as well as that of "Sesotris" in an adjacent valley, also so sculptured, overlooked the district in which are the multitudinous tumuli near the Gygean Lake; and the fortifications and camps of the acropolis of Sardis corresponded with those already referred to in position and design; and the same features were found at Ephesus, Smyrna and Pergamos in a striking degree, and extended, as shown by careful drawings and diagrams, onwards eastward as far as China. Considering the skill and thought required to plan such enormous figures with any regard to proportion, and seeing that all the figures had similar accompaniments, the author concluded by observing that they seemed to him to have been the result of a practice and culture transmitted with concurrent customs by way of the Pacific from one continent to another.

THE CROSS AND THE CRUCIFIX. By CHAS. WHITTLESEY, of Cleveland, Ohio.

[ABSTRACT.]

THE purport of this paper was to recommend great caution on the part of archæologists in giving religious significance to letters, symbols or hieroglyphics that may resemble more or less some forms of crosses or quasi-crosses. It was held that the *crucifix* should not be confounded with the cross, which assumes a great variety of forms which were discussed.

The Christian symbol originated at the crucifixion, and had an upright part extending above the arms of the olden cross. In nearly all ancient inscriptions there are letters or symbols that have a greater or less resemblance to the ancient cross. Unless something more is found than a resemblance of form, it is unsafe to draw inferences that they are connected with worship. The Runic characters furnish several that might be called crosses, but which are only letters.

Chief Deities in American Religions. By Albert S. Gatschet, of Washington, D. C.

[ABSTRACT.]

The science of comparative mythology is acquainted with the full religious systems of a *few* American religions only; the majority of them are known to us in a very imperfect, fragmentary manner. When we speak of *the religions* of American Indians, this term is not to be understood as bearing chiefly on the moral side of religion, or connecting morals with religious beliefs, as it is done in the monotheistic religions of the eastern hemisphere. With us a religious person is equivalent to a moral, honest person, and God is considered not only as almighty and omniscient, but also as the God of truth, forgiveness, and as the embodiment of moral principles. But rude and savage nations do not connect such ideas with their deities. Their deities represent the objects and powers of nature, as far as these have any influence on the welfare of man. They do not embody originally any moral principle, for the winds, clouds, rains and celestial bodies, which they represent, have nothing moral or immoral in them; they are purely physical objects or powers. To become objects of worship, the forces of nature have to be personified; that is, human souls, passions and powers of reflection have to be imputed to them. Man feels himself in his welfare dependent upon the powers of nature and if he thinks them to be animated with souls and minds like his own, he can at least cherish the hope to communicate with and perhaps to propitiate them in spite of their stern and threatening countenance or their cruel destructiveness. In this early stage of religious feeling men seek to turn the more obnoxious deities in their favor by offerings, sacrifices or ascetic exercises, while the spirits representing the powers of nature favorable to him do not enforce the same attention and are therefore neglected by the ungrateful crowd. This condition of religious ideas necessarily implies *polytheism;* but in adaptation to the objects worshipped by the natives of America, we had better call *polydaimonism* the mode of worship existing in the western hemisphere.

The nations, which by their climatic position are destined to reach a higher stage of culture, will, from the religious condition of polydaimonism, which is an imperfect anthropomorphism, ascend by gradual and easy steps to that of real anthropomorphism.

Man being the most perfect being in organic nature, the savage mind naturally regards his nature-spirits as human beings endowed with miraculous powers; or should he continue to see animal monsters in them, he at leasts invests these with human form and garb, arms them with the weapons of man and hears them speaking in the accents of human speech. The more perfectly his nature-spirits become anthropomorphized, in the same degree intellectual and *moral* qualities will be imputed to those regarded as favorable deities, mischievous and immoral characteristics to the obnoxious ones.

After a deity has fully assumed the human shape, it frequently figures as an apparently historical character, as a legislator, ruler in peace, culture-hero, teacher of plastic, fictile and agricultural arts, canalizer of rivers or originator of the calendar. In the tropical regions of America the culture-heroes generally have a duplicate deity of the same origin and representing the same powers of nature, but less approachable and less popular. When this euhemerizing process is completed, miraculous stories decrease in number, while in the earlier stages they made up the bulk of the myths of each spirit-deity. No religion is imaginable without miracles as its base and foundation. Miracles are the childlike efforts of youthful humanity to account for the mysterious working of the natural forces, and owe their origin to the earliest attempts of popular metaphysics and philosophy, of which religion forms a portion.

Ethnology in its present condition cannot assume that there is or was any people on the face of the earth that had no religion in the scientific acceptation of the term. All have some theories to account for the origin of all that exists; all possess definite ideas of the nature of their good and obnoxious gods and of the mode of propitiating or appeasing them, though we Christians prefer to call this superstition, not religion.

The purely physical base upon which the American religions are built up precludes the possibility of anything like our Holy Spirit, the Trinity, hell and heaven or the devil, although early navigators have not failed to discover the latter figure on many of the Indian idols. The Eastern (Asiatic, African) religions present comparatively solid systems when compared with the unsystematic character of the American religious beliefs. Many of the American religions, like those of the Aztecs and Peruvians, are the result of a

combination of the beliefs of several nations and therefore necessarily contain contradictions in their myths; but even where the mythology is the product of a single tribe it will show a lack of uniformity. The American deities are but symbols of the objects and powers of nature, as of the winds, the earth, the sky, sun and moon, night and day, the clouds, the rainbow, etc., and the eternal motions, *strifes* and *conflicts* of these reflect themselves in constant contradictions in the myths. The deities of the agricultural nations of the eastern hemisphere have left behind them long ago this early stage, and some of their number have, even in prehistoric times, developed into gods with predominantly mental and moral qualities, because at that early period they had already reached the full anthropomorphic stage. When, finally, by the invention of alphabets the composition of ritual and canonical books of religion had become possible, the beliefs then prevalent were consolidated into dogmatic systems, a powerful priest-class arose to uphold and teach them to the people, and the more highly civilized the nation was, the higher usually was the standard of morals ascribed by the canonical books to their principal deity.

In polytheistic and polydaimonic religions the deified objects and powers of nature are numberless. Objects and phenomena that are of little account to us often strike the imagination of the rude native through some peculiarity, and the result is deification. To every strange object is ascribed a body and a spirit or soul, which can live inside the body and outside of it and is regarded as something supernatural. Prominent deities often receive epithets and when these epithets are no longer understood by the people, because they contain an archaic form of the language, they often become gods or goddesses for themselves independent of the parent deity. Even the emblems of the divinities may become deities for themselves. By such and other causes the number of deities increase, by other causes a diminution is brought about and only the fittest will survive. Some are worshipped only in one province of a country, while others enjoy a more general worship.

In this motley crowd of new and ancient gods a few only rise to general prominence, and among these few one is considered as the principal deity. Many deities are supposed to be antagonists to the rule of this chief god, and though he is the most powerful among the gods, he is not the most popular god among men. Chief deities represent a combination of several powers of nature

united in one supernatural being; they are often placed in the background by one or two more brilliant and therefore more popular conceptions of the religious mind, and if his obnoxious or terrific qualities make him less attractive to mankind, his irresistible, demoniac strength will leave him the object of the most intense dread. His attributes as well as those of all other deities are the result of the climatic condition of the country or countries in which the religion took its origin. For American religions the term *chief deity* is preferable to that of supreme being or supreme god, for "*God*" is too anthropomorphic and "*supreme*" involves a position too high to be imputed to any of the deities found in American mythology.

After examining the religious beliefs of a large number of tribes and giving abstracts of the more important beliefs on this subject of chief deities, I arrived at the conclusion, *that the majority of all American tribes worshipping the sun regard the sun deity as their chief deity.* It is only the imperfect state in which tribal mythology is generally brought to our knowledge that prevents us from pushing our inquiries further in this direction. Even in the earliest prehistoric epochs the rudest savages must have comprehended that *to the sun* mankind owes not only its daily food and nightly rest, that the earth owes its fecundity to his balmy rays and the rains which cause the waters to flow everywhere; neither were they ignorant of the fact that man's life and existence are the sun's work and therefore worshipped in the solar spirit the creator of man, of plants, of animals, the earth and of the whole universe. In the eastern hemisphere sun worship has been largely superseded or placed into the background by other deities, chiefly of a moral or more abstract nature; but even there the features of this primeval cult are still perceptible to the trained eye in the religions of many tribes and nations.

The sun often appears in the form of two deities in the same tribal religion; one may be of an abstract, the other of a concrete aspect; or the deity is thought to rule in the sky, and his counterpart appears on the earth as a culture-hero with an apparently historical garb. The solar deity is always in conflict with the lunar god and the clouds, but not with the deity of the skies. The lunar god holds the position of a chief deity in a few religions, but never appears as the creator or maintainer of the whole earth and universe. When the sun is regarded as a female, the moon often,

though not always, appears as a male genius and in some tribes it is difficult to distinguish between solar and lunar god, because both celestial bodies are called *by the same term.*

Among the tribes, who worship the solar deity as their chief deity, are the following:

The Tupi god Tupan is a thunder god, but his attributes prove him to have been originally a solar genius. Tamo-i of the Guarani is "the old man of the sky," who is equivalent to the sun, like Tiri of the Yuracares, and the white-bearded Bótchika of the Muiska in Bogotá, while Dabaiba, on the isthmus of Panama, was probably a sun goddess also. In Peru three deities represented this celestial orb: Inti, Viracocha and Manco Capac; their worship originated at different epochs.

Tucapacha was the chief deity of the tribes of Michoacan. Coxcox of the Chichimecs. The Aztecs worshipped the sun in an abstract form in Tonatiuh, in a more concrete shape in Tezcatlipoca, who is the antagonist of the moon-god Quetzalcoatl, the latter corresponding to Kukulcan of the Maya religion.

A few of the Pueblo Indians of New Mexico worship Montézuma as their chief god, creator of the universe and culture-hero, while among the Californians the "chief above" or the "old man above" has a large number of votaries. Other Californians, like the Pit River Indians, regard the coyote-wolf as the principal deity and ascribe to him the creation of many objects in this world; the same may be said of a few Selish tribes on middle Columbia River, Washington Territory and Idaho, and of the Pápagos in Arizona. Coyote-worship is *originally* identical with lunar worship. The Wintún in northern California call their almighty power Noamkléstowa or "Great Spirit of the West." The main deity of the Klamath Lake and Modoc Indians of Oregon is K'muka'mtchiksh, the "primeval old man."

The Flathead-Selish of Montana pay their respects to a chief deity called Amotekēn, who is invisible and resides in the sky; he is possibly a genius of the skies, not of the sun. To the Niskwáli, Washington Territory, the chief deity is not the sun, but the moon, Slokwalm. The Haida Indians worship an abstract and a material sun-god, the former being the supreme ruler of the universe.

West of the Rocky Mountains we find among the majority of the tribes a deity called the *Master of Life*, the *Master of Breath*,

the *Great Spirit*, etc., and in addition to this, a series of solar deities differing among each other according to the tribes in which they are found. These are but the concrete expressions of what the *Master of Life* expresses in a more abstract form.

Sun deities and chief deities at the same time are: the Michabo of the Odshibwē, Areskui of the Hurons, Kutnähä or "midday sun" of the Shetimasha in Louisiana, the deer-god of the Floridian Timucua and many others. The sun-dance of the Sioux perpetuates sun-worship up to the present time, when a large proportion of them have become Christians.

INDIAN MIGRATIONS AS EVIDENCED BY LANGUAGE. By HORATIO HALE, of Clinton, Ontario, Canada.

[ABSTRACT.[1]]

IN this paper the author attempts to trace, by the evidence of language, reinforced to some extent by that of tradition, the course of migration which has been followed by the tribes belonging to some of the principal linguistic stocks of North America. The Cherokees are shown to belong to the Huron-Iroquois stock, but to have received accessions to their vocabulary from some other source. The Huron language is shown to be the oldest in form among the languages of this stock. The migration of the Huron-Cherokee tribes is traced in a course leading from the northeast to the southwest, that is, from the lower St. Lawrence to northern Alabama. The Dakotan stock is next considered. The Tuteloes of Virginia and North Carolina are shown to belong to this stock, and to speak a language which is older in its forms than the languages of the western Dakota tribes. The Algonkin tribes and languages are next examined, and the evidence is exhibited which shows that their migration probably flowed from Hudson's Bay and Labrador towards the south and west. The tribes of the Chahta-Muskoki family are noticed, and the fact is pointed out that their language, like that of the Cherokees, has apparently been modified by admixture with some alien speech. Some reasons are given for supposing that this speech was that of the Moundbuilders of the Ohio Valley. Traditionary and linguistic evidence is adduced

[1] This paper is published in full in the American Antiquarian for January and April, 1883.

to show that the Moundbuilders were conquered and partly exterminated by the Iroquois and Algonkins, and that the survivors, mingling with the Cherokees and Choctaws, caused great changes in the languages of those nations. The fact that the course of migration seems to have been from the Atlantic coast towards the interior is regarded as evidence that the ancestors of our Indian tribes were emigrants from Europe. In support of this opinion, reference is made to the close resemblance in structure between the Basque and the Indian languages. It is further suggested that if the Aryan intruders, entering Europe from the east, encountered and absorbed a population resembling the American aborigines, this fact would account for the great changes which the Aryan speech underwent in central and western Europe. It would also account for a very remarkable change which took place in the character of the intruding race. The Aryans, who in the east have always been a submissive and contemplative race, devoid of the idea of popular government, become in Europe a high-spirited, practical, and liberty-loving people. The conclusion is that the nations of modern Europe are a mixed race, forming a transition, in physical and mental traits, between the eastern Aryans and the aboriginal Americans.

THE KINSHIP SYSTEM AND MARRIAGE LAWS OF THE DHEGIHA, ILLUSTRATED BY CHARTS. By J. OWEN DORSEY, of Washington, D. C.

[ABSTRACT.[1]]

MARRIAGE laws based on kinship system, which, in turn, is founded on the gentile organization of the tribe. The last is affected by the customs pertaining to the buffalo hunt.

Chart I. The Omaha Tribal Circle, as formed during the hunt, and showing the gentes. The gentes divided into sub-gentes, for certain reasons. Father-right.

Chart II. "Consanguinities." Classes of the consanguinities of "SELF, a male."

Classes of the affinities of "SELF, a male," as shown in Chart III.

[1] The charts illustrating this paper are by necessity omitted.

Peculiarities of the charts caused by the marriage law: "SELF can marry any woman whom his wife calls *sister*, *brother's daughter*, or *father's sister*." Actual and potential kindred. The kinship formed by the calumet dance.

From the examples given in the charts, it is plain that among the Dhegiha, terms of kinship are used with latitude, differing from our ordinary acceptation of those terms. Then follows the full definition of each term. Women whom a man cannot marry. Men whom a woman cannot marry.

Women whom a man can marry. Men whom a woman can marry. Marriage of a brother's widow. Law concerning widows and widowers.

THE SUN DANCE OF THE OGALALLA SIOUX. By ALICE C. FLETCHER, of Cambridge, Mass.

THE sun dance is a tribal festival at which attendance is compulsory. The dance proper is a strictly religious rite, entered voluntarily and performed in fulfilment of vows made in sickness or trouble in order to secure health and prosperity. The dance proper is a centre about which many festivities cluster, incident to a great annual gathering. Thanksgiving and a desire for future benefits are the essential mental characteristics; the recognition of supernatural powers and dependence upon them, together with obligations of faithfully performing a vow are the religious characteristics; the exact observance of certain forms and rituals is demanded of the priests and their coöperation is needful to secure the full blessing sought through the ceremony by those taking part in it.

The festival generally occurs in the latter part of June or early in July and lasts about six days. The time is fixed by the budding of the *Artemesia ludoviciana*. The people camp in a circle, with a large opening at the east. In 1882, over 9,000 Indians were so camped, the diameter of the circle being over $\frac{3}{4}$ of a mile. The tent set apart for the consecrating ceremonies, which take place after sunset of the first day, was pitched within the line of tents, on the site formerly assigned to one of the sacred tents.

Any number of persons can dance, provided they have vowed to do so, but each year there can be but one Leader. All the dancers must conform to fixed rules and regulations. The dance may be divided into four degrees.

1. That which only the leader can pass through. He must vow, procure the articles for the ceremonial, abstain from forbidden things and actions, fast, dance, give away property, be scarified and tortured.

2. To vow, abstain, fast, dance, give away, be scarified and tortured.

3. To vow, abstain, fast, dance, give away, and be scarified.

4. To vow, abstain, fast, dance, and give away.

Twenty-two persons danced and passed these degrees as follows: First degree; the leader only. Second degree; one man. Third degree; seven men, one woman (I could learn of no woman ever having been tortured). Fourth degree; eleven men, one boy (11 or 12 years old).

The leader is required to make the vow, depend upon it for recovery, if sick, or deliverance, if in trouble. Soon after, he must set about fulfilling the duties of the vow. These are, to kill a buffalo, preserve the hide, which must never be taken into a lodge, keep the fat in skin bags, save the skull and two chips; lay away a red-stone pipe, the stem ornamented with porcupine quill-work; accumulate possessions to give away at the festival; abstain from running, swimming, loud talking, boisterous actions; live at peace and spend much time contemplating the fulfilment of his vow. In this manner the winter is passed. Early in June the leader selects a friend to be his assistant and these two proceed to one of the Holy-men, who has learned the ritual of the dance, and with certain ceremonies offer him the charge of the dance. From the moment of his acceptance, the leader places himself under the implicit guidance of this officiating priest. Every dancer chooses a lay assistant and a priestly guide. The leader's assistant, at the direction of the officiating priest, obtains an axe, awl, needle and four knives, all of which must be new; these are for the consecrating ceremonies and to be used in the rites. The officiating priest summons the people to the place selected for the festival.

The opening ceremonies begin with the entrance of the officiating priest and the leader, accompanied by their attendants, into the tent before mentioned. All are painted red and wear the buffalo robe in the sacred ceremonial manner, the hair outside, the head

on the left arm which is folded over the tail on the right arm. A fire is in the centre of the tent, a spray of sweet grass laid on it, the axe consecrated in the aromatic smoke and afterward used to cut the sod from an oblong space drawn on the earth back of the fire. The brown exposed earth is then mellowed, and a few live coals laid in the centre of the figure, sweet grass dropped on them, and through the smoke all the articles to be used during the ceremonial are passed for consecration, after having been first wiped with Artemesia. During this ceremony the priest chants the ritual, as he sits facing the east. The pipe is filled, smoked and passed, stem downward. The tent cloth is raised that all the people may hear the prayers chanted. These prayers are only spoken on this occasion, once a year.

The dancers every morning repair to this tent while certain rituals are sung, and in the evening they come here to dance and sing the songs of the festival. The people during these days enjoy feasts, dances, etc., incident to a reunion. The entire camp, however, is under the charge of men appointed by the priest to preserve order and carry out the regulations of the festival. On the fourth day the site of the pole is chosen by men set apart for the task, and the tree which is to serve as the pole is selected by young men appointed to the duty, and a man of known worth is designated to dig the hole for the pole. On the morning of this day the priest, leader and attendants repair to the selected tree, present the pipe, lifting the stem for the first time, after which a pipe is laid at the foot of the tree, a circle drawn on the ground near it, an offering of calico tied on the tree just below the branch which forks toward the east, and a man appointed to remain and watch the spot and suffer no living creature to pass over the consecrated ground. At noon the two singing societies repair to the place, and seven priests then light and smoke the pipe left by the leader, in accordance with special ceremonies. After this, five men and three girls are chosen to cut the tree, observing the order of procedure peculiar to this part of the festival. The tree is then struck with the sacred axe by the first young man, he recounting his claim to receive the honor; he leaves the axe sticking in the gash, the second young man pulls it out, lifts it, recounts his deeds, and strikes, leaving the axe in the tree, and so on by the five men, and the tree is thus girdled with gashes. The girls then fell the tree which must drop to the west. It is carried to the camp circle on a litter of sticks and must not be handled or stepped over.

Before it reaches the camp, the open circle is charged by all the able-bodied men in gala dress, their ponies decked and painted. They dash across the plain at full run, and skirt the outside of the line of tents. The pole is painted and the offerings tied upon it and erected with elaborate rites. After which a circular enclosure of boughs, having an opening at the east, and a roof formed of tent poles, resting on crotched sticks and covered with tent cloths. The poles and tent cloths are contributed by the different divisions of the tribe. The ceremony of peircing the children's ears is now in order, and a vast amount of possessions changes hands during these ceremonies. The rites incident to the puberty of girls take place at this time. On the following day at noon the embowered circle about the pole is charged by men on foot, who make four advances, led by the chosen warriors, and at the fourth charge, pass out at the eastern entrance. Soon after, the sun dancers enter, naked to the waist, with embroidered widths of cloth reaching nearly to their ankles and belted at their waists. Their feet are bare, on their heads are crowns of Artemesia with horns cut from rawhide and painted blue. In their hands are sprays of Artemesia. The leader carries in his arms the buffalo skull; with the priest followed by their attendants, they slowly pass round the circle by the south to east, the skull being placed opposite the pole and the entrance. The space behind the skull is occupied by the leader and officiating priest, the other dancers with their priests and attendants are to the right and left. The dancers have fasted, tasting neither food nor water, since the time the pole was cut, and they now dance, at intervals during the rest of the day and night and until after noon of the following day. When they dance they use a whistle which is made from the wing bone of an eagle, ornamented at one end by a peculiar, soft feather used in religious ceremonies. They blow this whistle to the rhythm of the song which is sung by the societies, accompanied by a drum made of a hide dried for this occasion.

On the morning of the sixth day, the buffalo skull is painted with a sign in blue color, indicating the four winds. An oval space is marked out on the earth behind it, the sod cut off and laid to the east of the oval; upon the top of this heap the skull is placed. The soil within the oval is then mellowed and a figure similar to that painted upon the skull is cut upon it, and into it tobacco is dropped. The soil of the oval and the skull are painted

with dashes of red ochre. A rest for the pipe is set up, just behind the oval, the bowl of the pipe put on a buffalo-chip, the rest supporting the stem. Artemesia is spread back of this and another buffalo-chip set further back to receive the ashes. Sweet grass is placed in given spots and offerings of food placed on the ground near the skull. At this time all the dancers are painted according to the visions of their respective priests. Scarifications are next performed. After noon, the leader is led to the pole, when according to certain ceremonies the flesh is punctured a little above the nipples by a wedge-shaped knife, and a stick having one end embroidered with porcupine quills is inserted. To this skewer the raw hide rope fastened to the pole is secured, and the man is led out toward the east until the rope is taut. A large amount of goods are then given away, and many ceremonies take place to insure him a speedy release. When a certain song is started, he must put his whistle in his mouth and bracing himself pull with all his force, until he shall tear the flesh loose. When one side gives way he raises his hand, palm upward to the sky, as a sign of thanks for the deliverance. With one man the struggle lasted nearly twenty minutes. When these tortures are completed the festival is over.

THE DAVENPORT TABLETS. By HORATIO N. RUST, of Pasadena, Cal.

[ABSTRACT.]

BELIEVING that the pictures on the Davenport Tablets lack evidence of great antiquity, and are the work of the Indians who were recent Moundbuilders, I took representations of the Tablets to the Dakotas, showing them to several of the older members of the tribe, telling them how they were found, and asking what they meant. The answers were in each instance substantially the same and without hesitation, viz.:

That the picture of the "mound" represents an earth lodge, in which a dance was being held.

The "prostrate forms" represented those persons who were overcome by the efforts and excitement of the dance.

The "curling smoke" arose from a fire in the lodge, indicating that the dance was held in cold weather.

The "moon and stars" signify that the dance was conducted in the night.

The "upright marks" around the lodge represent a fence of sticks set in the ground. A common custom among them to-day.

The irregular markings which some persons have tried to interpret as evidence of a written language, were simply ornamental markings conveying no intelligence.

Similar earth lodges destroyed by time and other causes have formed many mounds and groups of mounds, which now may be seen upon the benches all along the Missouri River.

A FIND OF CEREMONIAL WEAPONS IN A FLORIDA MOUND, WITH BRIEF NOTICE OF OTHER MOUNDS IN THAT STATE. By A. E. DOUGLASS, of New York, N. Y.

[ABSTRACT.]

WHILE considerable attention has been paid to the sand and shell mounds upon the western or Gulf coast of Florida, as well as those upon the St. John's river and its tributaries, the mounds distributed in equal abundance along the Atlantic border of the State have up to the present time almost escaped notice. Gradually they are fading away under the corrosion of agriculture and the elements, and the more serious evil of the curiosity of relic hunters, intent simply upon the acquisition of some object of pecuniary value, indifferent meanwhile to the characteristics of the mound they destroy, and to the facts attending the locality of the objects obtained which may invest them with peculiar archæological value. Relics of metal have gone into the melting pot, and others of stone have been broken in the handling and finally thrown away and lost.

Employing a few weeks in each winter during the last four years, I have taken what I may call an enthusiastic interest in recording the characteristic features of and in excavating more

than forty of these sand mounds, comprising all that I was able to hear of, on a seaboard of about one hundred and twenty miles in length, and averaging twelve miles in width. This district, commencing about ten miles south of the mouth of the St. John's river, extends to the southern limit of Mosquito Lagoon, and comprises the eastern halves of St. John's and Volusia Counties. Like the seaboard of North and South Carolina and Georgia, the coast of Florida presents a continuous line of outlying islands or long and narrow peninsulas, wholly or partially separated from the mainland by salt water lagoons which afford navigable water channels for the entire distance of three hundred miles from the mouth of the St. John's to Jupiter Inlet, except when interrupted by narrow tongues of land connecting with the mainland which interpose in the aggregate about twelve miles of land carriage.

The *shell mounds* in the district I have explored are mostly found upon the coast islands or peninsulas, while the *sand* mounds are, with very few exceptions, found upon the mainland, and generally near to streams of fresh water, which, draining the swamps, hammocks and pine barrens of the back country empty into the salt lagoons. The relics found in the sand mounds were quite abundant in St. John's County, but became more infrequent as I entered the northeastern border of Volusia County. The mounds themselves, however, gradually increased in dimensions, averaging north of the Halifax river not over ten feet in height, while those south of Mosquito Inlet ranged in height from twelve to twenty-three feet. Another remarkable fact was the prevalence of the celt, sometimes abundantly, in the St. John's County mounds, while south of the northern extremity of the Halifax river, in an examination of eighteen mounds, not one of these implements appeared.

The first sand mound I had the pleasure of excavating was what was called the Sanchez Mound, about eight miles north of St. Augustine, on a point of land formed by the confluence of the North river and Guano Creek. Though of moderate dimensions, nine feet in height with a base diameter of seventy-five feet, it produced a most abundant yield of celts (24) in connection with many other interesting objects. Four of these celts I venture to submit for your inspection as samples of more than one-half the number, the remainder being but little inferior except in condition and by reason of injury. The workmanship of these objects could hardly

be excelled even with the superior tools we now possess, nor their symmetry more than equalled by the skilled artisan of the present day.

We estimated that there were over twenty original burials in this mound, if we may so term deposits of bones made after years of exposure to the elements had wasted all but the most substantial. These were deposited as nearly as we could ascertain about two feet above the original level of the ground, and occupied a circle of eighteen feet in diameter. Each cluster of bones was surmounted by the skull, and the whole mass encrusted with red paint which discolored the sand an inch or two around them, and proved a useful guide to the workmen in detecting their proximity. Only two flint spearheads were found, and it may be well to note that, in arrow and spearpoints combined, the aggregate product of forty-one mounds was less than twenty. This is a remarkable fact when we consider that the Spanish historians, without exception, make no mention of stone implements other than arrowheads in use by the Indians at the time of the discovery of Florida, while the abundance of celts exhumed in this and other mounds would indicate that at some previous time *they* were in general use. None of these celts were found in juxtaposition with the bones, but were scattered singly throughout the interior of the circle at depths from the summit plateau varying from two to six feet. No beads were found in this mound, but in others they were occasionally abundant and were generally found in quantities laid immediately upon the skull which capped each deposit of bones.

The most singular and interesting discovery, it has been my good fortune to make during these explorations, occurred while excavating Banner Mound, known generally throughout the country as Mound Oswald. This is situated upon a tongue of elevated land at the intersection of Tomoka creek with the Halifax river in the northeast section of Volusia County. This creek drains a large expanse of swamp lying some twelve miles east of the town of Volusia (upon the St. John's river) and is navigable from its mouth for a distance of ten miles for vessels drawing five feet water. Running for this distance nearly due north, it intersects the Halifax river, whose course is from northwest to southeast, at a very acute angle, and the point thus formed was under continued cultivation from early English times, say 1770 up to the Indian wars

of 1837–42, when the settlers were finally driven off, and since when it has been deserted. It is now covered with a magnificent park-like growth of oaks, palmettoes and pines. The surface slightly undulating is elevated on the average ten feet above the level of the water, and about a mile and a half from the extreme point forms a series of parallel ridges, possibly the vestiges of an ancient sea beach. Upon the western termination of one of these ridges, this mound was raised, the constructors, as is frequently the case, availing of the natural elevation to give a more imposing height to the structure on one side at least. Hence, while on the north its perpendicular height appears as fourteen feet, on the south where the natural depression has been increased by excation for materials, its height appears as twenty-four feet. On the north the slope of the mound rises abruptly at an angle of forty-five degrees, while on the south it rises gently to the summit at an angle of thirty degrees. The plateau on the summit (common to most of these larger mounds) was slightly convex and twenty-eight feet in diameter. The southern half of this plateau was covered with a dense scrub, while the centre and northern area was crowned with palmettoes, oaks, hickories and sweet bay, all of great size. A trench twelve feet long, and from three to five feet wide had been excavated by some previous explorer from the southeastern edge of the plateau to a point about eight feet north of the centre. Commencing with a depth of three feet it gradually descended to a depth of eight feet at its terminus. The fruits of this enterprise, undertaken as I understood some years before by a party attached to the Coast Survey, I never learned; had the result been fortunate no doubt it would have been continued over the entire surface; possibly therefore it proved a failure. The labor of clearing the surface of a large mound of the growth of scrub and timber is very considerable, and, without doing this, it is exceedingly difficult to estimate the true position of the centre toward which the excavation must usually be directed as likely to produce the best result. After clearing the plateau of trees (a work of severe labor and occupying several hours), we commenced a trench along the edge of the plateau, twenty feet in length, four in width and the same in depth. Occupying this, the six men were then faced toward an objective point on the opposite edge of the plateau, and we proceeded to excavate a triangular pit, descending as we advanced till at the centre we reached

a depth of fourteen feet. Before doing this, the surface of the mound was carefully raked over and the debris examined, as objects lightly buried are occasionally exposed by the wear and tear of storms for centuries, and might, without this precaution, be again buried. In this instance we found a stone ornament designed as we supposed to be used as the brooch for a necklace of shell. Our labor the first day was otherwise quite fruitless, but on the morning of the second day one of my crew, practised in the work, detected by the sound upon his spade a substance foreign to the sand, and dropping the implement went to work with his hands, and quickly unearthed a beautiful ceremonial weapon popularly known as a banner stone. Groping about the spot with his hands he encountered one after another until five had been brought to light all in perfect condition. They were lying upon the same plane, but a few inches from each other, and fourteen inches only beneath the surface. It was not till two hours later, that, working upon a lower level, he unearthed three more of the same objects. These latter were found immediately below the first five at a further depth of three feet, and were placed vertically one above the other, separated by a film or thin stratum of sand. With one exception these eight objects are as fresh and new, save a slight discoloration, as when they left the maker's hands. They are the only objects of the class I have ever found in the Florida mounds, nor have I heard of the discovery of any in the state. This fact coupled with the number in this find, and the evidently studied design in their arrangement, warrants me in considering them the stock of a manufacturer and trader in such objects, who had availed of this mound to conceal his store, while he singly disposed of them, or else finding his safety risked had sought to hide them until he could again venture into the neighborhood and resume his trade. All these objects, as I before said, are, with one exception, absolutely perfect. They might, save for a slight discoloration, have left the makers' hands but yesterday, so clean and finished and unblemished do they appear. This exceptional one, however, is plainly many centuries older. It is covered with an amount of patina only to be acquired by exposure for a vast period of time. Five of these objects are of this special pattern, and one of its characteristics is common to all the specimens, that is, the ridge which accommodates the perforation is on one side *rounded*, on the other *flattened*. It seems a fair in-

ference after inspecting these objects, that the maker becoming possessed of this ancient specimen used it as a pattern for the rest, and may have used it for many another.

This specimen (fig. 1) consists of two semicircular flanges, or

FIG. 1.

wings or blades of thin stone, starting on opposite sides from a thicker ridge perforated throughout its extent. From edge to edge of these flanges the distance is five and one-fourth inches, the length along the axis or ridge is four inches, the perforation throughout

FIG. 2.

the length of the ridge is one-half inch in diameter and absolutely true. Two others of this pattern are about the same dimensions, and two others are slightly smaller. Again, two other specimens represent what may be called the double battle-axe pattern (fig.2).

The flattened edge of the blades forbids any idea of their having been intended or used for any warlike purpose. The distance from one edge to the other is, in one of these objects, five and a quarter, and in the other, five inches. These blades or flanges contract in width as they approach each other and merge into an axis or perforated ridge two and one-fourth inches in length, seven-eighths of an inch in thickness, while the perforation through its length is one-half inch in diameter. These two similar objects laid athwart each other would fairly represent a Maltese cross. The eighth specimen (fig. 3) differs from the first five, only in suppressing the ridge on one side of the object altogether, and thus having one surface absolutely flat, while a rounded ridge shows upon the other. The peculiarity of all these objects is the possession of this ridge flattened upon one side and rounded upon the other. Of one hundred and fifteen objects belonging to the class of cere-

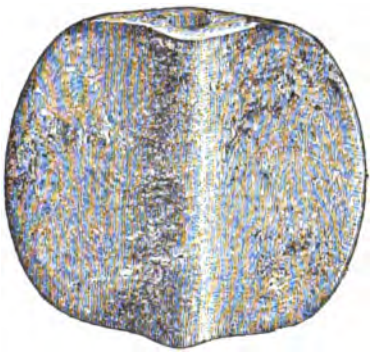


FIG. 3.

monial weapons in my possession, these and one other in a fragmentary condition alone present this characteristic. It is remarkable how from the condition and arrangement of this deposit one can trace the mode in which the trader buried these objects, and mark his ingenuity as he continued to conceal and render secure a part, if not all, of his hoard.

Selecting for the purpose a mound which the local tribes would be little likely to disturb, he proceeded hastily to excavate a cavity wherein he could spread them at length. He needed an implement for the purpose, and the ancient specimen before you answered most admirably. Observe how the attrition of the hand holding it has polished the patina of the one flange, while the

other presents a roughened edge fractured and splintered through the overlying patina by the use to which he put it, and showing in the fracture the natural color of the stone. But, as he digs, his ingenuity finds display in separating the hoard into two parts, so that if one should be discovered the other part might escape detection. After clearing a space sufficiently long and wide to permit of his spreading the five at length, he sank in the centre a circular hole three feet in depth, the extreme length of his arm and implement, and here he deposited three of the objects one above the other, as they were found, filling up the hole with sand to the level above. All that remained was to spread out the five remaining specimens in the trough prepared for them, and heaping the sand above them by means of the same ancient tool, he finally thrust it into the end of the trench, and there it was my good fortune to find it with the rest, some four centuries, perhaps more, afterwards. It remains but to say that the mound proved entirely destitute of burials either original or intrusive, nor were any other objects of interest found. We worked diligently until our excavation had reached the point aimed at on the opposite edge of the summit plateau, but without result. I would say briefly that this whole district must in ancient times have supported a most abundant population. About two miles to the eastward upon the opposite shore of the Halifax river, a mound of shell débris extends for a distance of nearly ten miles along the shore. Its average elevation is about fifteen feet, and its width at base one hundred and fifty feet. It is clothed with matted vegetation upon the sides, and crowned with noble oaks and pines upon its ridge. Many other shell mounds and fields are found in the neighborhood, all witnessing an abundant population in the unknown past.

RECENT ARCHÆOLOGICAL DISCOVERIES IN VERMONT. By G. H. PERKINS, of Burlington, Vt.

[ABSTRACT.]

NOT far from Middlebury, Vt., some peculiar specimens have been found under conditions which make it probable that they are the oldest implements yet discovered in the state. The surface soil at this locality is a clay-loam about a foot in depth.

Under this is a yellow subsoil. An apple tree in the orchard of a farmer was uprooted and in clearing away the débris stone implements were discovered. This led to further investigation and by persevering efforts quite a collection of objects was obtained. These were in the yellow subsoil from 18 to 24 inches below the loam and from 30 to 36 inches below the surface sod. There was no evidence that the soil had been disturbed or that the objects found had been deposited in graves. The specimens are themselves peculiar, being unlike any others found in Vermont. The material is in most cases a brownish siliceous stone from which "turtle backs" and other large and rather rude objects had been flaked. Ground implements, as celts and other like objects also have been found and a short cylinder of hard-burned pottery. Another interesting locality which I visited last summer proved to be the site of a village. Old settlers near by say that fifty years ago many skeletons were found, but none can be found at present. However, from all the cultivated fields which border what is known as Bristol pond in Addison county, great numbers of implements have been obtained. These fields are many of them on hillocks and the soil is in some of them clay, in others sandy, and in either case there are few stones of any sort except angular fragments brought by the former occupants. These are of all sizes from masses weighing eight or ten pounds to small flakes, the latter predominating. Over considerable areas the ground is completely filled with flakes and the total amount of material which must have been transported from a considerable distance to this village is very great. Hammerstones are, or rather were, for the region has been thoroughly looked over, so abundant as to be gathered by the basketful, and I presume that first and last a small wagon load of these implements has been carried off from this locality. The flaked implements are peculiar in their large size and rudeness. The material is almost uniformly gray quartzite. It seems probable that the large flaked implements were used as hoes or spades. It is difficult to guess at any other use to which they may have been put. Well ground celts and axes, pestles, amulets, banner stones, etc., occur and also finely flaked knives and arrowpoints, but by far the greater number of specimens found are either rather rudely made "turtle backs," or triangular or lanceolate objects from three to six inches long and two to four inches wide. Frag-

ments of pottery of very good quality have also been found and one bone object, the tyne of a deer's horn notched at the base as if for the attachment of a cord. It is not supposed that this village site was occupied in very remote times—there is no evidence of that—but yet it must have been before the occupation of the country by white men, since had there been a settlement there then, some tradition or account of it would remain until now.

TITLES OF OTHER PAPERS READ IN SECTION H.

A SCHEME OF ANTHROPOLOGY. By O. T. Mason, of Washington, D. C.

ON THE COMPARATIVE PHONOLOGY OF FOUR SIOUAN LANGUAGES. By J. Owen Dorsey, of Washington, D. C.

NOTICE OF A COLLECTION OF SIOUX WEAPONS AND ARTICLES OF DRESS. By G. H. Perkins, of Burlington, Vt.

EXHIBITION OF A COLLECTION OF CHIPPED-STONE IMPLEMENTS FROM THE PACIFIC COAST. By H. N. Rust, of Pasadena, Cal.

WHO BUILT THE MOUNDS? By P. R. Hoy, of Racine, Wis.

WHO MADE THE NATIVE-COPPER IMPLEMENTS OF NORTH AMERICA? By P. R. Hoy, of Racine, Wis.

MONUMENTAL AND ART REMAINS IN THE LAKE REGIONS OF OHIO, PENNSYLVANIA AND NEW YORK. By Wills De Hass, of Washington, D. C.

ARCHÆOLOGICAL EXPLORATION: PROGRESS OF DISCOVERY. By Wills De Hass, of Washington, D. C.

MOUNTAIN ANTIQUITIES. By Wills De Hass, of Washington, D. C.

GEOLOGICAL TESTIMONY TO THE ANTIQUITY OF MAN IN AMERICA. By Wills De Hass, of Washington, D. C.

"PAGAN" IROQUOIS FESTIVALS. By Erminnie A. Smith, of Jersey City, N. J.

BELIEFS AND SUPERSTITIONS OF THE IROQUOIS INDIANS.[1] By Erminnie A. Smith, of Jersey City, N. J.

A FEW DEDUCTIONS FROM A "DICTIONARY OF THE TUSCARORA DIALECT." By Erminnie A. Smith, of Jersey City, N. J.

HOME LIFE AMONG SOME OF THE NORTH AMERICAN INDIAN TRIBES. By Alice C. Fletcher, of Cambridge, Mass.

THE BLEACHING OF THE ARYANS. WAS IT DONE BY THE LAW OF REPETITION; THE ACCUMULATION OF EFFECTS? By Virginia K. Bowers, of Newport, Ky.

INFLUENCE OF CLIMATE OF CANADA ON EUROPEANS. By Wm. H. Hingston, of Montreal, Canada.

STONE IMPLEMENTS OF BOMOSEEN AND CASTLETON RIVER VALLEYS. By John McNab Currier, of Castleton, Vt.

A STONE-GRAVE IN ILLINOIS[2]. By Charles Rau, of Washington, D. C.

ON COPPER IMPLEMENTS AND ORNAMENTS FROM NORTH AMERICA.[3] By F. W. Putnam, of Cambridge, Mass.

THE CONTENTS OF EIGHTY-FOUR STONE-GRAVES AT BRENTWOOD, TENN.[4] By F. W. Putnam, of Cambridge, Mass.

ACCOUNT OF THREE MOUNDS EXPLORED IN OHIO AND TENNESSEE.[4] By F. W. Putnam, of Cambridge, Mass.

DISCOVERY OF THE REMAINS OF A LOG STRUCTURE BELONGING TO THE STONE-GRAVE PERIOD IN TENNESSEE.[4] By F. W. Putnam, of Cambridge, Mass.

[1] Will be printed in full in Second Annual Report of the Bureau of Ethnology Smithsonian Institution.

[2] Published in the American Naturalist, 1883.

[3] Published in full with illustrations in 15th Report Peabody Museum of American Archæology and Ethnology, Cambridge, 1882.

[4] The substance of these three papers will be given in the 16th Report of the Peabody Museum of American Archæology and Ethnology, Cambridge, 1883.

SECTION I.

ECONOMIC SCIENCE AND STATISTICS.

PAPERS READ.

STATISTICS IN AGRICULTURE. By J. R. DODGE, of Washington, D. C.

[ABSTRACT.]

THE offices and utilities of statistics in agriculture are indicated in this paper. Among these are: — the effort to measure the extent of national resources, contrast the actual with the possible in production, to weigh the effect of over-production in reducing prices, to show the error of reliance on uncertain markets, and mark the correlation of the industries, and the progress of the sciences in their application to rural arts. It shows how the emigrating farmer is guided by the light of statistics in seeking a home, choosing a location suited to his new circumstances, deciding whether to sell or store his crops at harvest, and in successive seasons whether to enlarge or to restrict the area of a given crop.

It states that orderly and scientific presentation of present and future accumulations of results of feeding experiments, that reveal the mysteries of animal physiology, will soon bear an important part in solving the problem of successful meat production. In view of the fact that the increase of value of all feeding material is greater than the advance in price of the product, thus reducing the margin of profit, this has become a vital question — how to produce a larger percentage of meat from a given measure of nutrients in feeding. Data from present and future experiment stations will be useful in this work, especially in showing the loss in flesh-making by exposure, by irregularity in time or quantity of feeding, by excess of succulent feed at one season followed by a ration of peculiar harshness and dryness during another period. Such statistics may harmonize the discrepant views of chemists and feeders upon the value of ensilage and the utility of the silo, and show the physiological necessity for variety, and possibly teach how meat may be made in winter as well as in summer, on hay and grain as well as grass, and eventually teach how two pounds of meat may be made where one was made before, and on a proportionally less quantity of nutriment.

Statistics have also been useful in reducing the overweening egotism of the cotton interest, which once assumed the regency of production and the dictatorship of exchange. Now the cereals have assumed the lead in exportation, and the products of the plebeian swine equal in value half of the exports of the patrician crop. A revolution is in progress, aided by statistics, showing the superior economy in cotton production through its subordination to diversification and rotation in a general system of cropping, by which the cost of culture is reduced, and the net profit of production increased. The progress of the south is shown in agriculture, in social and commercial economies, and in manufacture of cotton. The prediction is made of the increase of this industry to a practical equality with the north and Europe in consumption of the raw product.

The effect of the coördination of the facts of sheep-husbandry upon that interest is given, showing that the results of breeding in increasing weight and density of fleece and capacity for flesh-taking have been followed by extension of flocks and increase of price; and that, on the contrary, the exhibit of sudden reduction of wool had swept the pastures as with a besom of destruction — as in 1868, when four millions of peltless carcases were rendered for their tallow. The causes are slight that lead to the reduction of numbers and the abandonment of this industry — so sensitive is it to fluctuations of price — and the necessity for a uniform tariff policy is emphasized by these facts. The increase of wool-growing, during twenty years, is a striking fact — from 75,000,000 pounds to 300,000,000, and from a value of $20,000,000 to $80,000,000. Then the United States produced scarcely half that was manufactured, now fully four-fifths, the imported fifth being largely of low-priced carpet wools.

The use of statistics in determining whether agriculture is progressive or retrogressive is referred to, and the conclusion reached generally favorable to progress. Wheat culture is exhibited at some length, as illustrative of several aspects of this progress. The remarkable fact in its history is a doubling of the area in fifteen years, while in thirty years the increase in production has been nearly four hundred per cent, an increase, not only keeping pace with the increase of the population, but furnishing more than twice the supply *per capita*, viz.: 4.33 bushels in 1829, 5.5 in 1859, 7.28 in 1869, and 9.2 bushels in 1879. The facts of distribution

are shown: only twelve states and two territories produced a surplus; the majority of the states failed to produce their own bread, and the causes of their failure in the several sections are duly shown. Illinois produced the largest quantity in the census year, followed in order by Indiana, Ohio, Michigan and Minnesota. The latter state produced *per capita*, 44 bushels to each inhabitant, Oregon 42, California 34, Nebraska 31. The movement of wheat-growing westward is thus shown as a percentage of the whole crop:—

	1849.	1859.	1869.	1879.
Atlantic Coast	51.4	30.7	20	12.8
Central Belt	43.3	54.6	49	49.9
Trans-Mississippi	5.3	14.7	31	37.3
	100	100	100	100

The production of wheat in five years has been enormous — an average of 425 million bushels per annum, of which (with twenty millions drawn last year from the average reserve in hands of farmers) 233 millions were consumed in the United States, 145 millions exported, and between fifty and fifty-one millions used as seed. The utility of statistics in fixing prices and failing speculation is offered, and it is intimated that in the last year of partial failure, a saving to farmers of twenty million dollars was made by the publication of national and state crop statistics.

NOTES UPON SOME METHODS AND RESULTS OF COLLECTING STATISTICAL MATTER BY MAIL. By CHAS. W. SMILEY, of Washington, D. C.

[ABSTRACT.]

STATISTICS are frequently laughed at, usually pushed aside as "dry" and sometimes analyzed to the serious discomfort of the author. At other times they are extolled and made the basis of the most important action. I am quite sure that these various treatments are usually just. Accurate and truthful statistics are very scarce and, when found, very valuable. Figures often do misrepresent terribly notwithstanding the charitable maxim that "figures never tell lies."

Whoever has prepared statistics or tested them knows that the great and fruitful cause of bad statistics is in bad methods of

collecting the data. The methods are not revealed on the face of the results and hence the insidiousness of this cause. Is it not therefore pertinent at the very opening of the Section of Statistics and Economic Science to consider the subject of methods?

Mailing circulars does not constitute the most effective method, but if rightly managed it is cheapest. It is especially economical in the government bureaus where postage costs nothing. The most effective method is by personal visitation of skilled agents. This, from its expense, is usually out of reach of private institutions and often out of reach of government officers. The method of personal visitation I could extol very highly from my own experience and from close examination of results of that kind of work performed by others, but the present purpose is to treat solely of the collection of material by mail. I will describe, as illustrative of the best methods, an extensive correspondence carried on in the office of the U. S. Fish Commission.

In July, 1879, it was proposed by Gen. F. A. Walker, superintendent of the Tenth Census and Professor Spencer F. Baird, Commissioner of Fish and Fisheries, to ascertain something of the extent and nature of the fish trade and consumption of fish throughout the entire U. S. In consequence there was prepared a three-paged circular with blanks for answers and containing 18 brief but comprehensive questions, such as:—

1. Do fish constitute an important article of diet in your town and in the adjacent country?

2. Where is the supply obtained?

3. Check on the following list the kinds commonly to be seen in the markets: (list given)

4. What is the average retail price per pound?

5. What kinds of fish are taken from your ponds and streams? Check on the following list: (list given)

8. Are salted and smoked fish sold?

9. What kinds? Check on the following list: (list given)

11. Are oysters brought to your place? How are they brought; in shell, in tubs, in cans? What is the usual price?

16. Answer same questions for clams and lobsters.

18. Is fish guano in its various forms used by your farmers?

With this was sent to every Postmaster in the U. S., a circular letter asking the assistance of himself or some one familiar with the facts and accompanied by a circular from the Postmaster General instructing Postmasters to furnish such information as could

be given "without prejudice to the duties of their offices." An addressed return envelope accompanied each circular.

The result of this sending in general terms was as follows:

41,517 Postmasters were addressed, from whom inside of 200 days 16,996 replies were received or 41 per cent. At the expiration of 200 days it was deemed best to send again, to those who had not answered, precisely the matter as had been sent before. This was done with a very gratifying result. 24,521 Postmasters were addressed from whom inside of 200 days 13,233 replies were received. In the 680 days that have since elapsed, but 155 more replies have arrived. This is much less than one per cent. The net result of two sendings was to get 73 per cent. of the reports desired.

Sending.	Circulars sent out.	Returns within 200 days.	Per cent of the sending returned.	Per cent of desired reports received.
I	41,517	16,996	41	41
II	24,521	13,233	54	32
Total	66,038	30,229		73

I should have preferred a third sending at the expiration of 200 days from the second. I estimate that a third call on the remaining 11,133 officers might have produced about 6,000 more replies and brought the per cent of results up to 86 per cent of what was desired.

I have taken 200 days as the limit of returns. The number that arrive after that is very insignificant. But it will be valuable to know what results to expect earlier. Taking the total of 16,996 replies which came in the first 200 days, the percentage which came in each of the ten 20-day periods was as follows:

Twenty-day periods.	Number of returns.	Per cent of all.	Days that had elapsed from the time of sending.	Per cent of what was destined to come which had arrived each twenty-day period.
First	9680	57	20	57
Second	4711	27	40	84
Third	1313	8	60	92
Fourth	604	3½	80	95½
Fifth	314	2	100	97½
Sixth	165	1	120	98½
Seventh	72	½	140	99
Eighth	65	½	160	99½
Ninth	42	¼	180	99¾
Tenth	30	¼	200	100
	16,996	100	200	100

The return within 200 days of 97½ per cent of all that would arrive within 200 days shows that, when subsequent calls are to be made, 100 days is a very good limit to fix at which to send again. The receipts under the second call were even more precipitate than the first, being augmented slightly by the driblets from the first call. It was as follows:

Twenty-day periods.	Number of returns.	Per cent of all.	Days that had elapsed from the time of sending.	Per cent of what was destined to come which had arrived each twenty-day period.
First	7862	59½	20	59½
Second	2796	21	40	80½
Third	998	7½	60	88
Fourth	790	6	80	94
Fifth	338	3	100	97
Sixth	238	2	120	99
Seventh	112	1	140	100
Eighth	33		160	
Ninth	11		180	
Tenth	3		200	

The case under consideration was a semi-official call upon all the states and territories and the effect of the call upon the different sections was very different. While the total yield of returns was 73 per cent of the offices addressed, 95 per cent of the Dakota offices answered, and but 61 per cent of the Louisiana offices answered. The per cent of answers for each state or territory was as follows:

Dak.	95	Utah	86	Me.	81	Mont.	80
Ind. T.	89	Mass.	85	Nev.	81	Fla.	79
Wash.	88	Vt.	83	N. H.	81	Del.	66
Wyo.	88	Oreg.	83	N. Y.	70	Tenn.	65
Idaho	87	Ohio	76	W. Va.	69	Ky.	65
Cal.	78	Colo.	75	Md.	69	Ga.	65
Iowa	78	N. Mex.	73	Ariz.	68	Miss.	64
Neb.	78	Mo.	72	Ark.	68	S. C.	63
Tex.	78	Ind.	71	Pa.	67	Ala.	63
N. J.	77	N. C.	70	Wis.	60	Va.	62
Ill.	77	Mich.	82	Conn.	80	La.	61
R. I.	87	Kans.	81	Minn.	80		

It was to be expected that states would answer according to their degree of education and intelligence and this is no doubt an element, but some other element has entered in here to place five western territories ahead of the best states. My own opinion is that the semi-official endorsement of the Postmaster General had

a far greater influence upon the new sections of the country than upon the older; that new officers answered partly because of the official endorsement and that old ones have become somewhat accustomed to disregard such endorsements. But why Dakota exceeded Arizona 27 per cent and the Indian Territory exceeded New Mexico 16 per cent I am unable to suggest except the possibility of the loss of mails in transit, by fire, wrecking, robbing of mail coaches, etc.

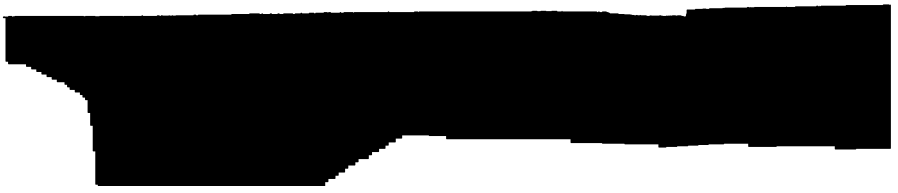
A grouping of the geographical sections yields some interesting results:

New England.		Great Plain Region.		Pacific Coast.		The North West.	
R. I.	87	Dak.	95	Wash.	88	Mich.	82
Mass.	85	Ind. Ter.	89	Oreg.	83	Kans.	81
Vt.	83	Wyo.	88	Nev.	81	Minn.	80
Me.	81	Idaho	87	Cal.	78	Wis.	80
N. H.	81	Utah	86			Iowa	78
Conn.	80	Mont.	80			Nebr.	78
		N. Mex.	73			Ill.	77
		Ariz.	68			Ohio	76
		Colo.	65			Ind.	71
Average	82	Average	82	Average	81	Average	78

Middle States.		South West.		South East.	
N. J.	77	Texas	78	Fla.	79
N. Y.	70	Mo.	72	N. C.	70
Pa.	67	Ark.	68	Md.	69
Del.	66	Ky.	65	W. Va.	69
		Tenn.	65	Ga.	65
		Miss.	64	S. C.	63
		Ala.	63	Va.	62
		La.	61		
Average	69	Average	68	Average	67

RECAPITULATION.

	Offices Addressed.	Ans. received in 680 days.	Per cent which answered.
New England	3176	2616	82
Great Plain Region	1381	1137	82
Pacific Coast	1528	1236	81
The Northwest	13157	10239	78
Middle States	7074	4905	69
The Southwest	8622	5865	68
The Southeast	6579	4386	67
Total	41517	30384	73



For quantity, these results were entirely satisfactory. The quality of the replies was of all grades. A very large per cent, however, bore internal evidence of truthfulness. A wise discretion was needed in the compilation but with skilled compilers excellent results could be produced. A discussion of methods of compilation, eliminating errors, etc., would be interesting but take too much time in this paper.

Let it not be supposed that these excellent results can be obtained only by government machinery. The official nature of this correspondence was a great aid, but I have obtained even better results from college men upon matters relating to their colleges and from specialists concerning their specialties. I therefore feel free to protest against the careless and inefficient work of this sort so often done by both public and private enterprise. Patience and perseverance in wise methods may not be all the qualifications that are necessary for good statistical work, but these are indispensable. Some of the suggestions to be made for success in collecting material are the following:

I. Make the questions very clear, concise and as few as possible. (Better send twice than totally break down by too long requests.)

II. If but one class of persons can be addressed from each of whom an answer is especially desired, send to all a first issue, to all who do not answer in 100 days a second issue, and to all who do not answer in 100 days more a third issue.

III. If more than one class of persons can give facts, address all of each class and after about 75 days address the delinquents a second time.

IV. Do not vary the matter sent the second time. Let it be an exact duplicate and be sent just exactly as if it had not been previously issued.

V. Leave blank lines between the questions so that no other paper is needed for reply, and if it can be put on a postal furnish it, even more for the sake of uniformity than for inducing people to reply.

VI. Always enclose an addressed envelope or postal for reply and provide that there be no expense to the respondent for postage.

VII. Remember that more or less of your circulars will be misdirected, lost in transit, fall into wrong hands, arrive during the

absence or sickness of your correspondents, be crowded aside for later answer or to get some needed information and then inadvertently overlooked, and so, do not lose faith in human nature but rather rejoice that amid so many contingencies you can get the material at all.

Exhibition of some Statistics of College men. By Charles W. Smiley, of Washington, D. C.

[ABSTRACT.]

These statistics relate to the graduates and non-graduates of Wesleyan University, especially the latter. They cover fifty years ending with 1881. The elaborate methods of collecting the data were described. Copies of eight tables were distributed to the members present. These are to be published in full in the Alumni Record of the college. The following summaries may be given to illustrate their scope.

I. Table showing the Number in each Class, the Living, Dead, etc.

Classes.	Number of Non-graduates.	Number believed to be living.	Not certain whether living.	Numb'r dead.	Died before ent'r'ng life work.	Died since ent'r'ng life work.	No. of graduat's and non-graduat's	Per cent. who were not graduates*
1832–41	159	59	10	90	14	76	334	
1842–51	199	106	16	77	18	59	452	
1852–61	178	123	10	45	11	34	457	
1862–71	175	130	3	42	21	21	435	
1872–81	224	199	0	25	17	8	548	
Totals.	935	617	39	279	81	198	2226	
Per Ct.	100	66	4 1-6	29 5-6	8 4-6	21 1-6		42

*This ran as high as 58 per cent in 1834 and 1867, and exceeded 50 per cent in eleven years; it ran as low as 21 per cent in 1875, and as low as 28 per cent in 1859.

II. Table showing Time of Leaving this College, subsequent places of Study, Degrees, etc.

Classes.	No. in the Classes.	Year of leaving this College.				No. who on leaving entered		No. who have attained Degrees.						
		Senior.	Junior.	Sophomore.	Freshmen.	Other Colleges	Profess. Sch'ls.	A.B. or A.M.	M.D.	LL.B. or B.C.L.	LL.D. or D.C.L.	D.D. or S.T.D.	Ph.D.	Miscellaneous.
1832–41	159	26	33	52	48	32	12	38	12	2	5	4	0	0
1842–51	199	17	57	63	62	46	23	47	16	6	2	3	0	1
1852–61	178	8	42	60	68	60	28	56	21	6	2	2	0	1
1862–71	175	14	42	55	64	40	15	33	15	8	1	1	1	4
1872–81	224	19	38	70	97	35	18	20	7	10	0	0	1	6
Total.	935	84	212	300	339	213	96	194	71	32	10	10	2	12
Pr. Ct	100	9	23	32	36	23	10	21	8	3	1	1		1

III. TABLE OF FAMILY STATISTICS.

CLASSES.	MARRIED.					NOT MARRIED.	CHILDREN REPORTED.				
	Once.	Twice.	Thrice.	Total.	Unkn'n.		In families	Alive.	Dead.	Total.	Av'ge per family.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	311	44	3	357	113	93	422	1.102	251	1.353	3

IV. TABLE OF NATIVITIES.

Years	Me.	N. H.	Vt.	Mass.	R. I.	Ct.	N. Y.	N. J.	Pa.	South.	West.	Foreign.	Unknown.
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	8	8	4	1	1	2	3	3	2	1	1	1	3

V. PARENT OCCUPATION.

| [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |

VI. Occupation since leaving College.

Occupation.	Number engaged.	Years spent.	Average years in each occupation.
Bishops	2	33	17
Ministry, Methodist Episcopal	136	1706	13
Protestant Episcopal	20	369	18
Congregational	11	107	10
Baptist	6	74	12
Presbyterian	4	75	19
Other sects	9	69	8
College Presidents	7	46	7
College Professors	17	157	9
Seminary Instructors and Tutors	10	70	7
Teaching professionally	94	1157	12
Teaching temporarily	100	264	3
Judges	7	68	10
Lawyers	144	2312	16
Physicians	82	1176	14
Dentists	2	46	23
Journalists	47	426	9
Authors	3	32	11
Manufacturers	37	611	17
General business	194	2365	12
Scientific work	2	46	23
State Legislatures	33	62	2
Members of Congress	4	14	4
Customs, Revenue, and Postal offices	11	83	8
Other public offices	19	47	2
Farming	66	868	13
Superannuated	8	53	7
Regular Army and Navy	11	70	6
Civil Engineers	10	127	13
Artists	4	32	8
Gentlemen of leisure	2	62	31
Mexican War	2	4	2
Federal Army,[1] 1861–5	92	220	2
Confederate Army, 1861–5	6	16	3

[1] Died during, or from injuries received in, the war, 22.

VII. Colleges entered, by 213 non-graduates of Wesleyan University, after leaving this College.

The Colleges entered, and the number who went to each, were as follows:

[illegible]	45	Asbury, Lafayette, Ohio	
Yale	33	Wesleyan, Madison, St.	
Amherst	14	[illegible]'s College, Univer-	
Princeton	14	sity of Pennsylvania,	
University of the City of		University of Virginia,	
New York	11	and Williams, each 2	24
Brown	11	Andover, Bates, Colby,	
Dartmouth	7	Hobart, Emory,	
Rutgers	7	Franklin and Marshall,	
Harvard and Union, each		Illinois Wesleyan, Iowa	
6	12	Wesleyan, Kansas, North-	
Syracuse, West Point, and		western, New York, Rut-	
University of Michigan,		gers, University of Ver-	
each 5	15	mont, U. S. Naval Acad-	
[illegible]	4	emy, and Wooster,	
Bowdoin and Dartmouth		each 1	15
each 3	6		
[illegible]			213
[illegible]			

[illegible] of the Census of 1880. By Calvin [illegible]

[illegible] of the United States

by age and sex, classified as Native White, Foreign White and Colored,[1] according to the United States Census of 1880.

AGES.	ALL CLASSES.			NATIVE WHITE.		FOREIGN WHITE.		COLORED.	
	Total.	Males.	Females	Males.	Females.	Males.	Females	Males.	Females
All ages.	50,155,783	25,518,820	24,636,963	18,609,265	18,234,026	3,521,635	3,038,044	3,387,920	3,364,893
Under 1 year.	1,447,963	734,024	713,959	617,307	595,430	2,989	3,061	113,728	115,468
1 year.	1,256,956	638,032	618,924	534,030	515,491	5,020	5,066	98,982	98,367
2 yrs.	1,427,086	726,038	701,048	601,683	579,272	6,893	6,752	117,462	115,024
3 yrs.	1,381,274	697,209	684,065	577,733	563,881	7,797	7,801	111,679	112,383
4 yrs.	1,401,217	712,406	688,811	587,440	565,513	8,557	8,435	116,409	114,863
Under 5 yrs.	6,914,516	3,507,709	3,406,807	2,918.193	2,819,587	31,256	31,115	558,260	556,105
5 to 9 years	6,479,660	3,275,131	3,204,529	2,694,308	2,625,324	61,803	60.894	518,930	518.311
10 to 14 years	5,715,186	2,907,481	2,807,705	2,361.832	2,280,260	120,740	117,699	424,909	409,746
15 to 19 years	5,011,415	2,476,088	2,535,327	1,965,748	2,007,090	184,320	194,492	326,020	333,745
20 to 24 years	5,087,772	2,554,684	2,533.088	1,945,279	1,928,938	274,038	254,217	335,367	349,933
25 to 29 years	4,080,621	2,109,741	1,970,880	1,472,960	1,397,625	365,094	306,022	271,687	267,233
30 to 34 years	3,368,943	1,744,308	1,624,635	1,128,308	1,088,315	419,769	342,662	196,231	193,458
35 to 39 years	3,000,419	1,527,159	1,473,260	920,264	938,963	432,957	356,308	173,938	177,989
40 to 44 years	2,468,811	1,243,773	1,225,038	726,832	751,013	384,831	327,959	132.010	146,066
45 to 49 years	2,089,445	1,078,695	1,010,750	621,164	609,787	340.863	290,078	116,668	110,885
50 to 54 years	1,839,883	936,702	873,181	538,133	512,672	318,045	259,042	110,524	101,467
55 to 59 years	1,271,434	674,927	596,507	403,260	378,541	203,820	166,294	64,847	51,672
60 to 64 years	1,104,219	584,858	519,361	345,575	321,270	170,841	139,022	68,442	58,469
65 to 69 years	725,876	379,498	346,378	243,803	232,245	98,028	83,333	37,667	30,800
70 to 74 years	495,442	250,001	245,441	165,062	164,476	59,590	55,019	25,349	25,946
75 to 79 years	281,035	138,601	142,464	94,598	99,574	31,106	29,590	12,897	13,300
80 to 84 years	146,362	67,941	78,421	44,492	51,949	15,156	15,930	8,293	10,542
85 to 89 years	49,835	21,908	27,927	14,858	19,425	4,419	5,096	2,631	3,406
90 to 94 years	16,100	6,351	9,749	3,486	5,357	1,270	1,690	1,595	2,702
95 to 99 years	4,763	1,855	2,908	783	1,260	433	575	639	1,073
100 and over	4,016	1,409	2,607	237	355	156	207	1.016	2,045

[1] The colored population includes 105,465 Chinese, 66,407 Indians (not in tribal relations), and 148 Japanese, whose ages have not yet been separately compiled.

TITLES OF OTHER PAPERS READ IN SECTION I.

ON INTERNATIONAL STANDARD TIME. By E. B. Elliott, of Washington, D. C.

SUGGESTIONS OF ELECTRICAL UNITS. By E. B. Elliott, of Washington, D. C.

POPULATION STATISTICS OF THE CHEROKEES. By E. B. Elliott, of Washington, D. C.

ON CERTAIN GOVERNMENT SECURITIES. By E. B. Elliott, of Washington, D. C.

UPON THE INVESTMENT OF LABOR AND CAPITAL IN FOREST CULTURE AS COMPARED WITH OTHER PRODUCTIVE INDUSTRIES. By Franklin B. Hough, of Lowville, N. Y.

THE JESSUP COLLECTION TO ILLUSTRATE AMERICAN FORESTRY, IN THE MUSEUM OF NATURAL HISTORY, CENTRAL PARK, NEW YORK. By Albert S. Bickmore, of New York, N. Y.

ON METHODS OF OBTAINING REGISTRATION OF VITAL AND OTHER STATISTICS OF THE OMAHA AND COGNATE TRIBES OF INDIANS. By J. Owen Dorsey, of Washington, D. C.

REFERENCES TO SOME OF THE ECONOMIC AND SCIENTIFIC PRINCIPLES IN TREE-GROWTH AND TREE-DESTRUCTION. By James Hyatt, of Stanfordville, N. Y.

STANDARD TIME FOR NORTH AMERICA. By Winslow Upton, of Washington, D. C.

EXPERIMENTAL PLANTATION OF EUCALYPTUS NEAR ROME. By Franklin B. Hough, of Lowville, N. Y.

COÖPERATION OF OBSERVATIONS FOR MAINTAINING ACCURATE TIME. By J. Rayner Edmands, of Cambridge, Mass.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

WEDNESDAY, AUGUST 23, 1882.

THE thirty-first annual meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE was called to order in the William Molson Hall of McGill University, Montreal, shortly after ten o'clock, by the retiring President, Prof. GEORGE J. BRUSH of New Haven, who introduced to the members the President for the coming year, Dr. J. W. DAWSON of Montreal.

President DAWSON then took the chair and spoke as follows:—

Ladies and Gentlemen of the Association:

I need hardly say how highly I esteem the honor you have done me in my election as your President for the present meeting. The unanimous election of a body like this to such an office is, in my judgment, the greatest distinction to which any scientific man on this continent can aspire, and I value it accordingly.

I have no doubt, however, that in the present case, the choice was in some degree determined by the wish to do honor to Canada, and to give to this meeting a character as thoroughly international as possible.

But this view of the reasons for the election by no means detracts from its value. On the contrary, it places the presidency of this association on a broader basis than that of any other office by extending over all this wide continent and including all that belongs to the powers that reign both at Washington and Ottawa. Science from her serene height thus overlooks all national boundaries and comprehends this whole world in her scope of vision.

It becomes your duty, therefore, for the time being, to merge the character of citizens of the United States or of Canada in that of cosmopolitan men of science. This is what I propose to attempt in endeavoring to perform the duties to which you have called me; and you will, therefore, kindly regard me not as a Canadian, or as an American, in the narrower sense of that term, but as the President of a society which, in meeting here, assumes a continental and international character.

THE PRESIDENT then requested the Anglican BISHOP OF MONTREAL to invoke the divine blessing on the Association's meetings, after which ceremony the PRESIDENT called on Dr. T. STERRY HUNT, Chairman of the Local Committee, who spoke as follows:—

In welcoming in the name of the Local Committee the American Association for the Advancement of Science to our city on the occasion of this, its thirty-first meeting, a crowd of memories rise to mind, many of which will be shared alike by citizens of Montreal and by my colleagues

of the Association. Twenty-five years have passed since this Association met here in August, 1857, under the presidency of Prof. James Hall, who honors us with his presence again to-day. Of the Local Committee of that time only four are now with us; Dr. Dawson, the President-elect of the Association, Dr. Hingston and Mr. Charles Garth, both members of the Executive Committee for this meeting, and myself. Those who were with us on that occasion will note great changes and great progress alike in the Association and in ourselves. Montreal was then a city of 60,000, and has to-day a population of 150,000 souls. The infant university, of which Dr. Dawson had just then assumed the direction, has under his wise care attained a vigorous and flourishing manhood, and the collections in natural science, gathered with few exceptions during his administration, now suffice to fill the great museum just completed and presented to the University by the generosity of a citizen of Montreal.

Not less remarkable is the growth of our Association, which has in the meantime increased more than fourfold, and now numbers nearly 2000 members. The history of this and its related associations demands a word in passing. Unlike national academies or royal societies these are popular bodies, inviting to their membership all who have an interest in the Advancement of Science, holding their annual meetings in different cities of their respective countries and so performing a kind of missionary work. The parent of all these is the German Association founded more than half a century since by the venerable Oken. Next in order of time comes the British Association, which held its fiftieth anniversary at York last year under the presidency of Sir John Lubbock, and to-day meets again at Southampton, with Dr. C. W. Siemens as president. Two of its honored past-presidents, W. B. Carpenter and Samuel Haughton, are with us, as our guests, to-day. Next in order of time comes our own Association, of which more anon; and last, but not least, the young French Association for the Advancement of Science, which was born with the new French republic, and met last year in Algeria, bearing back across the Mediterranean the torch of science to the soil of northern Africa, which ages before had been the cradle of arts and sciences. It was in May, 1881, that the French nation carried the war of science into Africa, well supported by allies from other lands, among whom was our distinguished Hungarian guest of to-day Dr. Szabó, of Budapest.

But to return to our own organization: the American Association, though it reckons its meetings only from the year 1849, has an earlier history. In 1842 there was formed a little band of workers under the name of the American Association of Geologists and Naturalists. To this body I, at that time a student in the University, was admitted on the occasion of its meeting at New Haven in May, 1845. The scope of this association was, however, not large enough. The claims of physics, of chemistry and of mathematics, all indispensable allies of geology and of natural history, soon made themselves heard in our meetings, and the success which had attended the German and British Associations already mentioned led to its reconstruction and enlargement with its present

organization and title. This was effected at Philadelphia in 1848, and with the subsequent history of the Association you are more familiar. Thirty-four years have thus passed, and we are to-day holding our thirty-first meeting. In its earlier days, the Association occasionally tried the experiment of two meetings annually, but the plan of a spring gathering was soon abandoned. Afterwards, during the late civil war, came an interruption of five years, during which the deliberations of our Association were silenced by the clash of arms, until its meeting in Buffalo in 1866; since which time its gatherings have been held annually.

From an early period in its history it was felt that this Association should be in its widest sense North-American. Science knows no political boundaries, and the policy adopted by our organization was continental. This was due in no small degree to the importance of the work of the Geological Survey of Canada, which, begun in 1842, had early drawn the attention of naturalists to this country. To this field of service I had come from my native Connecticut as early as 1847, and Professor James Hall, though persistently rejecting the repeated overtures which were made to him to join the Canadian survey, had at intervals given it the benefit of his great knowledge of American geology and paleontology. Hence it came about that in response to an urgent invitation from the people of this city the American Association for the Advancement of Science first met in Montreal in August, 1857, Prof. James Hall being president. The late Sir William Logan, at that time my chief, and the director of the Geological Survey, was then chairman of the Local Committee, while I was secretary of the Special Committee on Invitations.

And now twenty-five years have come and gone, and true to its continental character the American Association once more comes to Montreal to hold its annual meeting, having elected for its presiding officer the honored president of our University, and for its general secretary a representative naturalist from the province of Ontario. It has returned to find Montreal grown to a great metropolis, busy with manufactures and commerce, its wharves lined with ocean-steamers and ships of all nations; while it boasts at the same time that the growth of letters and science has kept pace with its material progress, and invites the members of the Association to visit its churches and asylums, its schools and its museums. It can only tell you with feelings of disappointment that the Geological Survey, with its valuable library and its extensive museum, has now left Montreal, having been last year removed to Ottawa, the seat of the federal government; while we at the same time regret that the duties of its present director, Dr. Selwyn, have, by calling him to the provinces of the far northwest, prevented him from meeting with us on this occasion.

The plan and scope of our Association, like that of its European sisters, is eminently popular, since it invites to its membership all those who by their attendance and their contributions are willing to show their interest in our objects. It is at the same time eminently conservative in its organization. Within the wide circle of its members, or associates as they might be called, is a select body of fellows, to whom alone, by the con-

stitution, is intrusted the government of the body. These are under the present revised constitution carefully selected from the members for their scientific attainments, and are alone eligible for office in the Association. Moreover, those who have had the honor of filling the chair of president of the Association are perpetual members of its governing body, the Standing Committee.

So much in my capacity of an original member, fellow and past-president of the Association, I have ventured to recall for the information alike of citizens and the younger members of the Association. To all its members who have now honored us with their presence I would say that the Local Committee, not unmindful of the importance of the American Association, and of the distinction conferred on this city in choosing it as a place of meeting, has spared no pains or care to do honor to its guests, and now bids you a hearty welcome.

The MAYOR OF MONTREAL was next introduced and welcomed the members of the Association to the city in the following words:—

Mr. President, Ladies and Gentlemen:

I feel highly honored at having the very pleasant duty to perform of welcoming those gentlemen from the different parts of America — from the Dominion of Canada and the United States — who have met here to discuss questions which interest the whole of humanity — a welcome, gentlemen, which the citizens of Montreal and the City Council have heartily voted to the different Associations of scientific men — the Forestry Congress, as well as this American Association for the Advancement of Science — who have met here and are now daily meeting to discuss questions of the utmost importance. I have every hope and confidence that your deliberations will result in great improvements in the different branches of Science. Mr. President, on behalf of the citizens of Montreal, I welcome all those who are attending the American Association for the Advancement of Science, the American Forestry Congress and the Society for the Promotion of Agricultural Science, and I am authorized to offer you the freedom of the city.

President DAWSON then read a letter from His Excellency THE GOVERNOR GENERAL, regretting his absence, and also announced that His Honor, Lieutenant-Governor ROBITAILLE had expected to be present to address a few words of welcome to the members, but had been unexpectedly called to a distant part of the Province.

Dr. THORBURN, of Ottawa, who was next called on, said that at a public meeting recently held in that city, steps had been taken to give a hearty welcome to those members of the Association who might visit the capital. Ottawa had many attractions and he felt sure that the members would be pleased with their visit and trusted that as many as could would favor his fellow-citizens with their presence on Saturday next.

The PRESIDENT then said:— It falls to me, gentlemen, to acknowledge on behalf of the Association, the welcome which you have given it. In

doing so, I have the advantage of knowing from personal observation the zeal and energy which have been displayed by the members of the Local Committee, and the enthusiasm kindled by the occasion in the minds of the citizens generally. The occasion is undoubtedly one worthy of the interest it has excited, not only in this and neighboring cities but in Canada generally. By its selection for the meeting of this Association, Montreal becomes for the time the scientific capital of North America, from which will go forth decrees more potent than those of parliaments, and to which will be turned the eyes of all interested in the progress of Science and the Scientific Arts. You do well to esteem highly the position thus conferred and to sustain it in the future by the actions of your own local institutions.

The fact that we can congratulate ourselves on this occasion in the presence of so many and so eminent men of science from the other side of the Atlantic is largely due to the exertions of the Local Committee and to the liberality of the citizens of Montreal. I had the honor in 1856 to be the spokesman of a delegation which went to Albany to invite this Association to hold the meeting in Montreal in 1857, which has been referred to. That meeting was a most successful one, though, no doubt, far inferior in numbers to the present. It benefited this Association and the interests of science, and gave a stimulus to the cause of scientific education and research in Montreal, the effects of which still remain. May the present meeting be still more agreeable and useful. Of the leading men of science who took part in the meeting of 1857 many have passed away: Henry, Logan, Bache, Pierce, Munroe, Billings and Morgan are among these. But many of the men who then stood high in science are with us to-day, and you now welcome a large number of men who have since that time risen into eminence, and of still younger men whose names will yet be widely known. To such young and rising men you should, and I know do, extend a welcome as hearty as to those who are their seniors and are better known to you. You welcome here to-day a much larger body than that which you received in 1857. This is evidenced not only by the larger number of members, but by the increased number of sections. In 1857 there were two sections and a sub-section. Now there will be no less than nine sections meeting simultaneously. I have only to add, on behalf of the Association, that the ordinary meetings of the sections, and those evening-meeting addresses which will be advertised, are open to all who may desire to benefit by them; and that the membership of the Association is open to all suitable applicants on very easy terms. Such entertainments and excursions as have been provided by the Local Committee or by local institutions and private individuals, are of course limited by the invitations these may issue, but I have no doubt they will be made as general as the circumstances will permit.

The PERMANENT SECRETARY announced that the financial report of the year had been presented to the Standing Committee and would appear in the volume of Proceedings. Several subscriptions had come in for re-

printing volumes of the Proceedings of the Association, among others one from Gen. William Lilly of $1,000 for reprinting volume twenty-six, and since he had come to Montreal he had received another contribution of $850 for a similar purpose from a member, who for the present wished to remain unknown. He also stated that one hundred and sixty papers had already been entered for this meeting and referred to the various sections.

Dr. T. STERRY HUNT, on behalf of the ART ASSOCIATION, invited the members to visit the Art exhibition.

The PERMANENT SECRETARY then stated that notices of the decease of the following members of the Association had been received since the last meeting:

THOMAS ALLEN, of St. Louis, Mo. Joined the Association at the 27th meeting. Died April 8, 1882.

ZACHARIAH ALLEN, of Providence, R. I. An original member. Died March 17, 1882.

JOHN G. BARNARD, of New York, N. Y. Joined at the 14th meeting. Died May 14, 1882.

GEORGE S. BLACKIE, of Nashville, Tenn. Joined at the 26th meeting.

ALBERT D. BRIGGS, of Springfield, Mass. Joined at the 13th meeting. Died Feb. 20, 1881.

LEVI S. BURBANK, of Woburn, Mass. Joined at the 18th meeting. Died Aug. 20, 1880, in his 52d year.

MRS. MARY H. CAMPBELL, of Crawfordsville, Ind. Joined at the 22nd meeting. Died Feb. 27, 1882.

FREDERICK COLLINS, of Washington, D. C. Joined at the 28th meeting.

J. M. CRAMP, of Wolfville, N. S. Joined at the 11th meeting. Born July 25, 1796. Died Dec. 6, 1881.

CHARLES F. CROCKER, of Lawrence, Mass. Joined at the 22nd meeting. Died in July, 1881.

E. A. DALRYMPLE, of Baltimore, Md. Joined at the 11th meeting. Died Oct. 30, 1881.

CALEB G. FORSHEY, of New Orleans, La. Joined at the 21st meeting. Died in Aug., 1881.

W. W. GLAZIER, of Key West, Fla. Joined at the 29th meeting. Died Dec. 11, 1880.

J. GOLDMARK, of New York, N. Y. Joined at the 29th meeting. Died in April, 1882.

GEORGE W. HAWES, of Washington, D. C. Joined at the 23d meeting. Born Dec. 31, 1848. Died June 22, 1882.

THOMAS POTTS JAMES, of Cambridge, Mass. Joined at the 22nd meeting. Born Sept. 1, 1803. Died Feb. 22, 1882.

LEWIS H. MORGAN, of Rochester, N. Y. Joined at the 10th meeting. Born Nov. 21, 1818. Died Dec. 17, 1881.

WILLIAM H. MUSSEY, of Cincinnati, Ohio. Joined at the 30th meeting. Born Sept. 13, 1818. Died Aug. 1, 1882.

CHARLES H. PAYN, of Saratoga Springs, N. Y. Joined at the 28th meeting. Died in 1881.

J. DUNCAN PUTNAM, of Davenport, Iowa. Joined at the 27th meeting. Born Oct. 18, 1855. Died Dec. 10, 1881.

WILLIAM BARTON ROGERS, of Boston, Mass. An original member. Born Dec. 7, 1804. Died May 30, 1882.

ELIHU ROOT, of Amherst, Mass. Joined at the 25th meeting.

WILLIAM SHEPPARD, of Drummondville, Quebec. Joined at the 11th meeting. Born in 1783. Died in 1867.

DAVID P. SMITH, of Springfield, Mass. Joined at the 29th meeting. Died Dec. 26, 1880.

CHARLES SPINZIG, of St. Louis, Mo. Joined at the 27th meeting. Died Jan. 22, 1882.

A. REMSEN THOMSON, of New York, N. Y. An original member. Died in Oct., 1879.

WILLIAM S. VAUX, of Philadelphia, Pa. An original member. Born May 19, 1811. Died May 5, 1882.

GOUVERNEUR KEMBLE WARREN, of Newport, R. I. Joined at the 12th meeting. Died Aug. 8, 1882, in his 64th year.

J. CRAIG WATSON, of Ann Arbor, Mich. Joined at the 13th meeting. Born Jan. 28, 1838. Died Nov. 23, 1880.

Mrs. G. O. WELCH, of Lynn, Mass. Joined at the 21st meeting. Died in June, 1882.

CHARLES M. WHEATLEY, of Phœnixville, Pa. An original member. Died May 6, 1882.

LEWIS E. WHITING, of Saratoga Springs, N. Y. Joined at the 28th meeting. Born March 7, 1815. Died Aug. 2, 1882.

This list includes the names of five original members, two past-presidents, and the treasurer of the Association.

The GENERAL SECRETARY announced that the Standing Committee recommended for membership in the Association the members of the Local Committee in Montreal and one hundred and sixty-nine other persons whose names were read to the meeting. On motion, the GENERAL SECRETARY was instructed to cast an affirmative ballot for the election of the parties named, who were then declared duly elected members of the Association.

The PRESIDENT announced that the following named gentlemen from abroad, who were present as invited guests of the Association, had been nominated for membership by the Standing Committee, and they were duly elected by the Association.

Dr. JOSEPH SZABÓ, Royal Counsellor, Professor of Geology and Mineralogy, Budapest, Hungary.

CHARLES WELD, Professor of Histology, University of Vienna.

Dr. WOLDEMAR KOWALEVSKY, Professor of Geology, University of Moscow.

Dr. RUDOLPH KŒNIG, of Paris.

Rev. SAMUEL HAUGHTON, Trinity College, Dublin.
GEORGE FRANCIS FITZ GERALD, Trinity College, Dublin.
W. S. MCCAY, Fellow of Trinity College, Dublin.
ALFRED HAMILTON ORMSBY, Barrister-at-Law, Dublin.
Rev. Prof. WILTSHIRE, King's College, London.
Dr. JOHN RAE, F. R. S., LL. D., Kensington, London.
WILLIAM B. CARPENTER, C. B., M. D., London.
Dr. J. W. GILBERT, Rothamsted, England.
JOHN COX, Warden of Cavendish College, Cambridge.
ERNEST H. COOK, Bristol University School, Bristol, Eng.
J. S. PHENÉ, LL. D., F. S. A., F. G. S., London.

The PRESIDENT also announced that the annual meeting of the British Association opened on the same day with this meeting.

The recommendation of the Standing Committee that the hours of meeting each day, except Saturday, should be from 10 A. M. to 1 P. M., and from 2.30 to 5 P. M., was adopted.

The General Session was then declared adjourned for the meetings of the Sections in their respective rooms for organization.

SECTION A. *Mathematics and Astronomy.*

In the absence of the Vice President, WM. HARKNESS of Washington, the Section was called to order by the Secretary, H. T. EDDY of Cincinnati, and W. A. ROGERS of Cambridge was elected temporary Chairman.

W. W. JOHNSON of Annapolis was elected to the Standing Committee, W. W. PAYNE of Northfield to serve on the Nominating Committee, and the following Sectional Committee was elected: G. W. HILL of Washington, LEONARD WALDO of New Haven, C. A. YOUNG of Princeton.

The Subcommittee, to act with the Vice President and Secretary in recommending to the Nominating Committee the Vice President and Secretary of the next meeting, was elected as follows: F. H. SHERMAN, Nashville, R. W. WILSON, Cambridge, C. H. ROCKWELL, Tarrytown.

The Section then adjourned until 2.30 P. M.

In the afternoon, in the absence of the Vice President, his address was read by J. R. EASTMAN of the U. S. Naval Observatory.

SECTION B. *Physics.*

Section called to order by Vice President T. C. MENDENHALL of Columbus, CHAS. S. HASTINGS of Baltimore, Secretary.

Fellow elected to Standing Committee: E. W. BLAKE, Providence.

Sectional Committee: C. K. WEAD, Ann Arbor, A. E. DOLBEAR, College Hill, H. A. ROWLAND, Baltimore.

To Nominating Committee: T. F. JEWELL, Newport.

Sectional Subcommittee: N. D. C. HODGES, Salem, J. B. MURDOCK, Annapolis, S. H. BRACKETT, St. Johnsbury.

Section adjourned to meet at 4 P. M., when Vice President MENDENHALL delivered his address.

SECTION C. *Chemistry.*

Section called to order by Vice President H. C. BOLTON of Hartford, ALFRED SPRINGER of Cincinnati, Secretary.

Fellow elected to Standing Committee: S. A. LATTIMORE, Rochester.

Sectional Committee: G. C. CALDWELL, Ithaca, E. W. MORLEY, Hudson, R. C. KEDZIE, Lansing.

To Nominating Committee: G. C. CALDWELL, Ithaca.

Sectional Subcommittee: A. A. JULIEN, New York, P. SCHWEITZER, Columbia, A. R. LEEDS, Hoboken.

Section adjourned to meet at 2.30 P. M., when Vice President BOLTON delivered his address.

SECTION D. *Mechanical Science.*

Section called to order by Vice President W. P. TROWBRIDGE of New Haven. In the absence of the Secretary, C. B. DUDLEY, J. BURKITT WEBB of Ithaca was elected to fill the vacancy. The other elections were deferred until the following day.

Section adjourned to meet at 4 P. M., when Vice President TROWBRIDGE delivered his address.

SECTION E. *Geology and Geography.*

This Section was called to order by Vice President E. T. COX of San Francisco. In the absence of the Secretary, C. E. DUTTON, the Section elected H. S. WILLIAMS of Ithaca to act in his place.

Fellow elected to Standing Committee: G. H. COOK, New Brunswick.

Sectional Committee: C. H. HITCHCOCK, Hanover, N. H. WINCHELL, Minneapolis, E. T. NELSON, Delaware.

To Nominating Committee: J. W. SPENCER, Paxton.

Sectional Subcommittee: P. W. SHEAFER, Pottsville, ALEX. WINCHELL, Ann Arbor, WILLIAM LILLY, Mauch Chunk.

Section adjourned to meet at 4 P. M., when Vice President COX addressed the Section, and an interesting discussion followed.

SECTION F. *Biology.*

Section called to order by Vice President DALL of Washington. In the absence of the Secretary, C. S. MINOT, WILLIAM OSLER of Montreal was elected to fill the vacancy.

Fellow elected to Standing Committee: A. J. COOK, Lansing.

Sectional Committee: J. A. LINTNER, Albany, W. J. BEAL, Lansing, H. F. BASSETT, Waterbury.

To Nominating Committee: L. F. WARD, Washington.

Sectional Subcommittee: THOS. MEEHAN, Germantown, B. D. HALSTED, New York, T. J. BURRILL, Champaign.

Section adjourned to meet at 4 P. M., when Vice President DALL delivered his address.

SECTION G. *Histology and Microscopy.*

Section called to order by Vice President A. H. TUTTLE of Columbus, ROBERT BROWN, jr., of New Haven, Secretary.

Fellow elected to Standing Committee: R. H. WARD, Troy.

Sectional Committee: A. B. HERVEY, Taunton, LOUIS ELSBERG, New York, R. HITCHCOCK, New York.

To Nominating Committee: J. D. HYATT, Morrisania.

Sectional Subcommittee: G. O. MITCHELL, Hanover, T. J. BURRILL, Champaign, C. C. MERRIMAN, Rochester.

Section adjourned to meet at 2.30 P. M., when Vice President TUTTLE delivered his address.

SECTION H. *Anthropology.*

In the absence of the Vice President, DANIEL WILSON of Toronto, the Secretary, OTIS T. MASON of Washington, called the Section to order.

Fellow elected to Standing Committee: P. R. HOY, Racine.

Sectional Committee: A. S. BICKMORE, New York, GARRICK MALLERY, Washington, N. S. TOWNSHEND, Columbus.

To Nominating Committee: HORATIO HALE, Clinton.

Sectional Subcommittee: J. O. DORSEY, Washington, GEO. H. PERKINS, Burlington, JOSEPH ANDERSON, Waterbury.

Section adjourned to meet at 4 P. M., when the address of Vice President WILSON was read by the Secretary, OTIS T. MASON.

SECTION I. *Economic Science and Statistics.*

Section called to order by Vice President E. B. ELLIOT of Washington, F. B. HOUGH of Lowville, Secretary.

Fellow elected to Standing Committee: JOSEPH CUMMINGS, Evanston.

Sectional Committee: J. R. DODGE, Washington, CHAS. W. SMILEY, Washington, E. B. WHITMAN, Cambridge.

To Nominating Committee: ELI E. TAPPAN, Gambier.

Section adjourned to 2.30 P. M., when Vice President ELLIOTT addressed the Section.

In the evening a General Session of the Association was held in the Queen's Hall, for the purpose of hearing the address of the retiring President of the Association, Prof. GEORGE J. BRUSH of Yale College, New Haven. President DAWSON introduced Professor BRUSH, who then delivered his address, after which the members were invited to repair to the assembly rooms adjoining, where a kind reception awaited them by the Local Committee represented by Dr. T. STERRY HUNT. Music was provided, and the rooms were handsomely decorated with flowering-plants, palms and ferns, while at one end of the main hall refreshment tables were placed, and several hours were passed in pleasant social intercourse.

THURSDAY, AUGUST 24.

The Association met in General Session at 10 A. M., in the William Molson Hall, President DAWSON in the chair.

The PERMANENT SECRETARY announced that an invitation had been extended to the members of the Association, by the directors of the MONTREAL AMATEUR ATHLETIC ASSOCIATION to visit their rooms and library.

The GENERAL SECRETARY read the list of names of sixty-one persons recommended for membership by the Standing Committee, making two hundred and thirty in all. On motion, the Secretary was directed to cast an affirmative ballot, and they were declared elected.

The Association then adjourned to meet in Sections. [The papers read before the several Sections are given in other parts of this volume.]

In Section A, a magnificent series of astronomical plates, from drawings by Mr. E. L. TROUVELOT, was placed on exhibition.

In Section C, the Chairman announced that, in accordance with the resolutions passed at the previous day's session, he had appointed a committee consisting of F. W. CLARKE, ARTHUR H. ELLIOTT and H. B. NASON, to discuss the feasibility of framing a list of chemical indices. He said their duty would be the discussion of one of the most important obstacles now in the way of chemistry. There being no uniform system of chemical indices, each chemist adopted a system of his own in recording the results of his investigations. When an eminent chemist chanced to die, the results of his life's experience and researches would follow him into his grave, and humanity would be deprived, perhaps, of the inestimable work of a life devoted to this science.

In Section D, DE VOLSON WOOD, of Hoboken, was elected to the Standing Committee, and G. LANZA of Boston, to the Nominating Committee.

In Section G, an invitation was read and accepted from the MONTREAL MICROSCOPICAL CLUB for Monday evening, from 8 to 10 P. M., for the practical demonstration of various modes of illumination.

In the evening the members were invited to the formal opening of the Redpath Museum, when the fine new building was presented by Mr. PETER REDPATH to the McGill University. After the ceremonies of presentation were over, Principal and Mrs. DAWSON held a reception in the Museum halls, to which the members of the Association and many citizens had been invited. During the evening Mr. Hovey's paper on "Cave Scenery" was given in the theatre, illustrated by lantern pictures.

FRIDAY, AUGUST 25.

The Association met in General Session at 10 A. M., President DAWSON in the chair.

The GENERAL SECRETARY read the names of fifty-two persons recommended by the STANDING COMMITTEE for membership in the Association, all of whom were duly elected. He also announced that the STANDING COMMITTEE had nominated Prof. W. A. ROGERS of Cambridge as Vice President for Section A, and Prof. ALEX. WINCHELL of Ann Arbor for Section H, and on motion the nominations were confirmed.

The STANDING COMMITTEE recommended that a committee be appointed to confer with committees of foreign associations for the advancement of

science, with reference to an international convention of scientific associations, the said committee to consist of T. STERRY HUNT, Montreal, ALEX. AGASSIZ, of Cambridge, Mass., and SIMON NEWCOMB, of Washington.

Also, that a committee be appointed to confer with a committee of the British Association on the annual records of science, said committee to consist of J. W. DAWSON, Montreal, F. W. CLARKE, Cincinnati, EDW. S. DANA, New Haven, H. W. MARTIN, Baltimore, and C. A. WHITE, Washington. Both these recommendations were adopted, and the committees appointed.

The following resolution was then proposed and adopted. Moved by Major JED. HOTCHKISS of Virginia, seconded by Prof. C. H. HITCHCOCK of Hanover, N. H.:—

Recognizing the relations of the late Prof. WM. B. ROGERS to this Association as one of its founders, his eminent and long continued services in the interest of science, and his world-wide reputation as a scientist, be it resolved, that the STANDING COMMITTEE of this Association be requested to appoint some member familiar with the life and labors of Professor ROGERS to prepare and read at the next annual meeting of this Association a suitable memoir of him, to be subsequently published in the Proceedings.

The Association then resolved itself into a meeting of Section B to listen to a paper "Upon the electrical experiments to determine the location of the bullet in the body of the late President Garfield, and upon a successful form of Inductive Balance for the painless detection of metallic masses embedded in the human body," by ALEX. GRAHAM BELL; it being understood that the other Sections would suspend their meetings until after the reading of this paper. At the conclusion of this interesting communication the thanks of the members were unanimously tendered to Professor BELL for his efforts in the cause of suffering humanity.

In Section C, the Index Committee reported the following resolution which was adopted:

Resolved, That a committee of five be appointed to *devise* and *inaugurate* a plan for the proper indexing of the literature of the chemical elements. Said committee to consist of the Chairman of the Section and four others to be appointed by him. The committee shall have full power to secure the coöperation of volunteers and be required to report at the meeting of the chemical Section in 1883.

F. W. CLARKE,
A. H. ELLIOTT,
H. B. NASON.

In Section I, Dr. HOUGH being obliged to leave for home, J. R. DODGE of Washington was elected Secretary of the Section.

On Friday afternoon a reception was given to the members by Mr. and Mrs. J. H. R. MOLSON in their house and grounds at Piedmont from 4 to 7 o'clock.

In consequence of the excursions to Quebec and Ottawa, there were no meetings of the Association on Saturday. Those who joined the Quebec excursion left by boat on Friday night, reaching Quebec early on Saturday morning, when, after breakfast on board the steamer, they were conducted by a Committee of citizens to visit the fortifications, the Citadel and the University. After this they embarked on another steamer for an excursion around the harbor, during which an excellent lunch was served on board, and was followed by addresses. The party returned to Montreal on Saturday evening by the North Shore Railway.

The Ottawa party left by early train on Saturday morning and reached the capital after a pleasant ride of four hours where they were met by the reception committee and conveyed to the various points of interest, after which luncheon was provided in the Drill Hall, accompanied by addresses of welcome and suitable responses by the members. The party returned to Montreal about midnight much pleased with the generous treatment they had received from the citizens of Ottawa. The excursion to Ottawa was conducted by Dr. J. W. DAWSON, and that to Quebec by Dr. T. STERRY HUNT.

MONDAY, AUGUST 28.

The Association convened in General Session at 10 A. M., the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of seventeen persons recommended by the STANDING COMMITTEE for membership in the Association, and they were duly elected.

The special committee on the proposed memorial to CHARLES DARWIN, reported as follows:—

Whereas a committee has been designated to coöperate with the English Committee for collecting subscriptions for a memorial to CHARLES DARWIN, of which American Committee Mr. ALEXANDER AGASSIZ is Treasurer: the STANDING COMMITTEE of the American Association for the Advancement of Science, confident that all American Naturalists, and all friends of science, will be glad to join in the furtherance of this object, hereby commend it to the Association, and request that all members of the Association and all others who desire to do honor to the memory of a noble character, a most sagacious investigator, and a single-hearted lover of knowledge for its own sake, should send any contributions they may wish to make, directly to Mr. ALEXANDER AGASSIZ, at Cambridge, Mass.

The Committee to memorialize the Legislature of New York for a new survey of Niagara Falls asked for further time, which was granted.

The Committee to obtain permanent funds for the Association and to advise with the Treasurer and Permanent Secretary on financial matters, was discharged.

The Committee on the best methods of science teaching in the public schools reported that they had no report to present this year, but the STANDING COMMITTEE desired that it should be continued. The Association agreed to this.

Prof. GEORGE J. BRUSH, of New Haven, reported on behalf of the Committee appointed to coöperate with the Committee of the American Philological Association in relation to the proper restriction of the degree of Ph. D. He explained that the degree of Ph. D., which it was first intended should be given only by examination, had been given very largely throughout the country as an Honorary Degree, and the JOINT COMMITTEE resolved to address a circular to the various colleges, representing these facts and asking them to restrict the conferring of the degree. This was accordingly done, the circular being prepared and sent to some 415 colleges throughout the country. He asked that the Committee be now discharged as it had fulfilled the duty for which it was appointed. The Association agreed to this and the Committee was discharged.

The Committee on State Geological Surveys reported progress, and desired to be continued. As this was recommended by the STANDING COMMITTEE, it was agreed to by the Association.

The Committee on Standard Time reported progress and asked for further time to complete their work. The STANDING COMMITTEE recommended that the request be granted, and this was agreed to.

The PRESIDENT announced that Dr. Elliott, who was not present, was to have reported on behalf of the Committee on Weights, Measures and Coinage, and also for the Committee on Registrations of Deaths, Births and Marriages. The STANDING COMMITTEE has recommended that these committees be continued. The Association agreed to their continuation.

The PERMANENT SECRETARY made a number of formal announcements, and the Association then adjourned to meet immediately afterwards in the various sections.

Section D having finished the reading of papers assigned to it concluded its sessions in the afternoon.

In the afternoon a garden party was given to the members of the Association by Mr. and Mrs. L. J. SARGEANT at their residence, Weredale, at the western end of Dorchester street.

In the evening at 8 o'clock, a lecture was delivered before a large audience, by Prof. A. MELVILLE BELL, of Kingston, Ont., at the Queen's Hall, on the subject, "Visible Speech."

A number of the members interested in Microscopy attended the meeting of the MICROSCOPIC CLUB held in the rooms of the Natural History Society. The numerous microscopes belonging to the members were in

use. At 9.30 the gathering was called to order by Mr. JAMES FERRIER, jr. Dr. CARPENTER was introduced, and in the course of a very interesting address spoke of and explained several microscopes which he had brought with him from England, and also explained certain objects as viewed under them.

There was also a large attendance at the ART ASSOCIATION conversazione and at the GYMNASTIC ENTERTAINMENT both held at the same time.

TUESDAY, AUGUST 29.

The Association met in General Session at 10 A. M., President DAWSON in the chair.

The GENERAL SECRETARY read the names of fifteen persons recommended by the STANDING COMMITTEE for membership in the Association, and on motion they were duly elected.

The STANDING COMMITTEE recommended that Prof. G. F. BARKER of Philadelphia be appointed to prepare a memorial of the late Prof. W. B. ROGERS; also that the following be appointed a committee on indexing chemical literature: Profs. H. C. BOLTON, Hartford, IRA REMSEN, Baltimore, F. W. CLARKE, Cincinnati, A. A. JULIEN, New York, A. R. LEEDS, Hoboken, both of which recommendations were adopted.

The GENERAL SECRETARY then announced that the time had arrived when the important business of selecting a place for the next year's meeting was to be settled and read the following invitations:

MINNEAPOLIS, MINN., AUGUST 15, 1882.

At a special meeting of the Minnesota Academy of Natural Sciences held this evening, the following resolution was carried:

Resolved, That the Minnesota Academy of Natural Sciences hereby cordially renew the invitation tendered the American Association for the Advancement of Science in the year 1881, to meet in this city in 1882; and hereby assure the Association in repeating for the coming year, 1883, that invitation seconded by the city and its various institutions, that its members will receive at the hands of our people the same generous hospitality promised them at that time.

It was further moved and carried that Prof. N. H. WINCHELL, a member of this Academy, be instructed to present the above resolution to the Association.

A. F. ELLIOTT, *President.*
C. W. HALL, *Secretary.*

IOWA AGRICULTURAL COLLEGE,
AMES, AUG. 19, 1882.

TO THE NOMINATING COMMITTEE OF THE A. A. A. S.,

GENTLEMEN:—In behalf of the scientific men of this College and State, I would urge upon you the desirability of selecting a western place of meeting for the Association in 1883. We will give you most cordial wel-

come. We hope that you will decide to meet in Minneapolis, where you will receive royal treatment.

Very truly,
C. E. BESSEY,
Vice President of the Iowa Agricultural College, and President of the Iowa Academy of Sciences.

The Secretary announced that the Association had also received several other invitations, which were of an informal character, and that the STANDING COMMITTEE had decided to recommend the acceptance of the invitation from Minneapolis.

The President, Dr. DAWSON, said that the STANDING COMMITTEE had given the matter every consideration, and they had been unanimous in their verdict. Prof. N. H. WINCHELL had attended their meeting and his description of the city and the facilities it would offer had given the committee every satisfaction.

It was voted unanimously to hold the next annual meeting at Minneapolis, Wednesday, August 15, 1883.

The GENERAL SECRETARY then read the following list of members recommended by the STANDING COMMITTEE to be elected Fellows of the Association.

Allen, Harrison, Philadelphia, Pa.
Alvord, Henry E., Easthampton, Mass.
Ashburner, Wm., San Francisco, Cal.
Atwater, W. O., Middletown, Ct.
Baker, Marcus, Washington, D. C.
Bartlett, J. R., Washington, D. C.
Blair, Henry W., Washington, D. C.
Borden, Spencer, Fall River, Mass.
Britton, N. L., New Dorp, N. Y.
Burrill, T. J., Champaign, Ill.
Call, R. Ellsworth, Des Moines, Ia.
Chamberlain, Wm. J., Columbus, O.
Chance, H. Martyn, Philadelphia, Pa.
Chandler, Seth C., jr., Cambridge, Mass.
Chester, Albert H., Clinton, N. Y.
Chittenden, Russell H., New Haven, Conn.
Claypole, Edw. W., Yellow Springs, O.
Collier, Peter, Washington, D. C.
Comstock, J. Henry, Ithaca, N. Y.
Dabney, Chas. W., jr., Raleigh, N. C.
Dolbear, A. E., College Hill, Mass.
DuBois, Aug. J., New Haven, Ct.
Dudley, Chas. B., Altoona, Pa.
Dwight, Wm. B., Poughkeepsie, N. Y.
Edwards, Wm. H., Coalburg, W. Va.

Flint, James M., Boston, Mass.
Frazier, B. W., Bethlehem, Pa.
Fuller, A. S., Ridgewood, N. J.
Gatschet, A. S., Washington, D. C.
Hale, Horatio, Clinton, Ont.
Hazen, W. B., Washington, D. C.
Hines, Chas. F., Carlisle, Pa.
Hodges, N. D. C., Salem, Mass.
James, Jos. F., Cincinnati, O.
Jewell, Theo. F., Newport, R. I.
Kendall, E. Otis, Philadelphia, Pa.
Kennan, George, Washington, D. C.
Langdon, F. W., Cincinnati, O.
Lanza, Gaetano, Boston, Mass.
Lazenby, Wm. R., Columbus, O.
Lilly, William, Mauch Chunk, Pa.
Love, Edw. G., New York, N. Y.
Macfarlane, James, Towanda, Pa.
Macloskie, George, Princeton, N. J.
McGee, W. J., Farley, Iowa.
McNair, A. R., Saratoga Springs, N. Y.
Mixter, Wm. G., New Haven, Ct.
Neale, Arthur T., New Brunswick, N. J
Nelson, A. B., Danville, Ky.
Peirce, C. S., Baltimore, Md.
Penhallow, D. P., Mountainville, N. Y.
Perkins, George H., Burlington, Vt.
Pilling, James C., Washington, D. C.
Platt, Franklin, Philadelphia, Pa.
Prentiss, D. Webster, Washington, D. C.
Reed, E. B , London, Ont.
Rockwell, A. P., Boston, Mass.
Ross, Waldo O., Boston, Mass.
Roland, Henry A., Baltimore, Md.
Russell, J. C., Salt Lake City, Utah.
Schanck, J. Stillwell, Princeton, N. J.
Seiler, Carl, Philadelphia, Pa.
Sestini, B., Woodstock, Md.
Sigsbee, Chas. B., Washington, D. C.
Smith, Edwin, Washington, D. C.
Snyder, Monroe B., Philadelphia, Pa.
Spencer, Jas. Wm., Windsor, N. S.
Stallo, J. B., Cincinnati, O.
Stevens, W. Leconte, New York, N. Y.
Stone, George H., Colorado Springs, Col.
Sturtevant, E. Lewis, South Framingham, Mass.
Swift, Lewis, Rochester, N. Y.

Taylor, Arthur F., Cleveland, O.
Thomas, Benj. F., Columbia, Mo.
Thomas, Cyrus, Carbondale, Ill.
True, Fred. W., Washington, D. C.
Trumbull, J. H., Hartford, Conn.
Van Dyck, Francis C., New Brunswick, N. J.
Walcott, Chas. D., Washington, D. C.
Wallace, William, Ansonia, Conn.
Warren, Cyrus M., Brookline, Mass.
Webster, H. E., Schenectady, N. Y.
Wharton, Joseph, Philadelphia, Pa.
Wheeler, Orlando B., Detroit, Mich.
White, I. C., Morgantown, W. Va.
Williams, H. S., Ithaca, N. Y.
Winsor, Justin, Cambridge, Mass.
Wright, Geo. F., Oberlin, Ohio.
Zentmayer, Joseph, Philadelphia, Pa.

These were balloted for and all duly elected.

The PERMANENT SECRETARY announced that the Standing Committee had decided to publish the papers read before the various Sections by title or abstract, unless a special recommendation should come from the Sections in relation to particular papers.

The Association then adjourned to meet in Sections.

Sections A, B, C, F and G finished the papers before them, and adjourned in the afternoon.

In the evening the members of the Association with a large number of citizens of Montreal met in the Queen's Hall to hear the lecture on THE TEMPERATURE OF THE DEEP SEA, by Dr. WILLIAM B. CARPENTER of London. President DAWSON was in the chair, and the officers of the Association with distinguished guests occupied the platform.

The PRESIDENT briefly introduced the lecturer, who on coming forward was greeted with applause.

Dr. CARPENTER, after some introductory remarks, said that the North Polar region was probably sea and covered with ice; while the South Polar region was probably land, or rather a group of islands covered with an ice-sheet extending out to sea and forming the great Antarctic barrier. If the whole area of the globe was divided into fifteen parts, it would be found that the land-surface covered only four, while the sea-surface covered eleven; or in other words the Land-surface was to the Sea-surface as four to eleven. But the proportion is very different when the mass of the water below the sea-level is compared with the mass of the land above it. If the whole land were levelled down to a uniform height, it would barely rise on an average a thousand feet above the sea-level. The average depth of the great ocean basins was however some 12,000 or 13,000 feet. Working out these figures, it would be found that the mass of the Land

was to the mass of the Sea as 1 to 36; or, in other words, the mass of the Sea *beneath* the sea-level was 36 times as much as the mass of the Land *above* the sea-level. Until very recent times, the speaker said, much difficulty had been experienced in ascertaining the temperature of this deep sea mass. Many observations had been made of bottom-temperature; but these were mainly local, and in many cases fallacious on account of the instruments employed. It has been found that at the enormous pressure to which they are subjected, the bulbs of the thermometers are compressed, and the mercury forced up into the tube; incorrect results being thus obtained. In 1868 he obtained from the British Admiralty the use of a vessel for carrying down dredging explorations to a greater depth than had been previously attempted; and being provided with the best deep-sea thermometers then made, he chanced, in using them, to stumble upon a most remarkable fact. A course had been shaped for the deepest water to be found near Great Britain; and a depth of more than 500 fathoms, or 3,000 feet, was obtained near the Faroe Islands. The thermometers were let down; and to his great surprise it was found that they fell to below 32 degrees. The doctrine among Physical Geographers at that time, was that the universal deep sea-temperature was about 39 degrees, which they considered to be the greatest density of sea water. At the same depth, within twenty miles of the same spot, a temperature of 45 degrees, 13 degrees higher, was found. This strange result excited his interest, as he considered that it had a most important bearing on fundamental questions of Physical Geography and Geology. He brought his results home, and urged upon the Admiralty the importance of ascertaining what effect different pressures would have upon the best-constructed thermometers. This was ascertained by the hydrostatic press; and it was found that at the pressure of three tons on the square inch (which was what their bulbs would have to sustain at a depth of about 2,400 fathoms) the thermometers went up from 10 to 20 degrees,—showing the fallacy of all past observations on deep-sea temperature. Half a ton pressure was found to send the instrument up some 2½ degrees; and from this he concluded that the temperature of 32 degrees which they had found was in reality 29½°. The next thing was to devise means by which this pressure on the thermometer could be obviated. This was done, at the suggestion of the late Prof. W. A. Miller, by simply enclosing the bulb of the thermometer in another bulb with some fluid between, but also including a vacuum; so that a slight reduction in the capacity of the outer bulb did not affect the real thermometer bulb. In all further observations these instruments were used, and gave the true temperatures. The result of these observations was, that the doctrine that 39 degrees was the general temperature of the deep sea was shown to be a fallacy; and it proved to have been propounded in ignorance of the fact long previously determined by the careful experiments of Despretz, that *sea*-water (unlike fresh water) continues to contract in cooling, down to its freezing-point at or under 28° Fahr. He was so much struck with his experience in the Faroe Channel, that he soon afterwards obtained another

steamer, and took observations with the new thermometers; when the real temperature of the cold area was found to be 29½ degrees, as they had expected. The bottom temperature of the North Atlantic, on the other hand was uniformly 35° Fahr. The cause of this was, that while the Faroe channel is an extension of the Arctic basin, the coldest Polar water cannot find its way into the basin of the North Atlantic, being kept back by ridges extending across the channels between Greenland and Iceland, and between Iceland and the Faroes.—The observations were carried down through the Bay of Biscay in 1870, and into the Mediterranean Sea. From all these observations the important conclusion was drawn, that the bottom-temperature of the deep sea is not determined by the latitude, but by the freedom of communication with either of the great Polar areas, permitting a vast underflow of Polar water towards the Equator, with a corresponding slow movement of the upper stratum from the Equator toward the Poles.

The lecturer then referred briefly to the subject of the Gulf Stream. This subject, he said, possesses much interest to persons on either side of the Atlantic. An enthusiastic American had asserted that America, by cutting a channel through the Isthmus of Panama, could entirely divert the Gulf Stream, and turn Great Britain into a howling wilderness. (Laughter.) The speaker assured the audience that this was no joke of his own, but had actually been propounded. But he thought he could show that there was not much truth in such a statement; and proceeded to explain what (in his view) the Gulf Stream does, and what it does not. The temperature of Great Britain is affected by a surface-flow of warm water flowing in a northeasterly direction. This serves to keep open all the harbors of Norway the most part of the winter, and was certainly of the greatest importance. Now, all marine surveyors had come to the conclusion that the Gulf Stream dies out as a current in the middle of the Atlantic; and that the remaining flow is mere surface-drift produced by the prevalence of southwesterly winds, which carries it northeast. It is demonstrable that in this passage any stratum of only 100 fathoms thickness (and the spread-out Gulf Stream current cannot certainly be deeper than this) would soon take the temperature of the air above it, so as to lose its special heating power. But the Challenger temperature-sections have shown that the Gulf Stream carries into mid-Atlantic an enormous body of warm water; which, in virtue of the higher specific gravity it derives from concentration, there sinks down, displacing colder water and thus makes the mid-Atlantic a storehouse of heat. The question then arises, How does this water get further? The speaker explained that this warm water is carried northeast by the general oceanic circulation, which brings to the British and Norwegian shores a stratum of warm water 500 fathoms deep, deposited as it were for their use in the mid-Atlantic. If they had not this General Circulation, the Gulf Stream would never come near their shores. If they had not the Gulf Stream, the General Circulation would do much less towards their warmth.—The speaker illustrated his remarks by numerous interesting diagrams.

At the close of the lecture, on motion of Professor JAMES HALL, a hearty vote of thanks was tendered to Dr. CARPENTER.

Afterwards a reception was given to the members of the Association by Mr. and Mrs. M. H. GAULT, in their beautiful grounds, which were brilliantly illuminated with Chinese lanterns, and handsomely decorated for the occasion. During the evening an admirable selection of music was rendered by Gruenwald's orchestra.

An "at home" was also given at the same time by Hon. Justice and Mrs. MACKAY.

WEDNESDAY, AUGUST 30.

The Association at 10 A. M. met in General Session in Molson Hall, President DAWSON in the chair.

The GENERAL SECRETARY read the names of ten persons recommended by the STANDING COMMITTEE for membership, all of whom were elected. This makes three hundred and twenty-five new members elected since the opening of the meeting in Montreal.

The STANDING COMMITTEE recommended the granting of the request from Section C, asking for a Committee to petition the Agricultural Department of the United States or Canada to print the paper on "Sources of nitrogen in the soil" by Messrs. LAWES and GILBERT, and the following Committee was appointed: GEORGE B. LORING, WILLIAM SAUNDERS, C. V. RILEY, E. B. ELLIOTT and O. T. MASON.

The STANDING COMMITTEE recommended the following amendment to ARTICLE VII of the Constitution.

"The Standing Committee shall also have power to exclude from the Association any member or fellow on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in their estimation made improper use of his membership or fellowship."

The consideration of this proposed amendment will take place at the next meeting.

Mr. ELLIOTT, Vice President of the Section of Economic Science and Statistics, presented the following report with the sanction of the Standing Committee:

"The Sectional Committee of the Section of Economic Science and Statistics, have considered the papers referred to them having reference to the Standard of Time, and recommend:

"First, That the Association should express its gratification at the passage of the joint resolution by the Congress of the United States, authorizing the President of the United States to call an international conference to fix on and recommend for universal adoption a common prime meridian, to be used in the reckoning of longitude and the regulation of time throughout the world.

"Second, They further recommend the appointment by the Association of a Committee to confer in such a manner as it may seem best with the

Convention named in the resolution of Congress, and also to represent this Association, as its delegates, in any other convention that may be called to consider the subject of the standard of time.

"It is understood that the important legislation on the part of Congress is due in no small measure to the earnest and well directed efforts of the American Meteorological Society and of the American Society of Civil Engineers. A communication from Mr. Sandford Fleming, representing the American Society of Civil Engineers, has been received by this Sectional Committee, and will be referred to the Committee which may be appointed.

"The following Committee on a primary meridian and an international standard of time is respectfully suggested, viz.: F. A. P. BARNARD, New York; C. A. YOUNG, Princeton; T. STERRY HUNT, Montreal; J. K. EASTMAN, Washington; CHARLES CARPMAEL, Toronto, and E. B. ELLIOTT, Washington.

The report was accepted and the designated committee appointed.

The PERMANENT SECRETARY then read the list of the officers proposed by the Nominating Committee for the next year, as follows:

President — C. A. YOUNG of Princeton, N. J.

Vice Presidents — Section A — W. A. ROGERS, Cambridge, Mass.; B — H. A. ROWLAND, Baltimore, Md.; C — EDWARD W. MORLEY, Cleveland, O.; D — DE VOLSON WOOD, Hoboken, N. J.; E — C. H. HITCHCOCK, Hanover, N. H.; F — W. J. BEALE, Lansing, Mich.; G — J. D. COX, Cincinnati, O.; H — O. T. MASON, Washington, D. C.; I — F. B. HOUGH, Lowville, N. Y.

General Secretary — J. R. EASTMAN of Washington, D. C.

Assistant General Secretary — ALFRED SPRINGER of Cincinnati, Ohio.

Secretaries of the Sections — Section A — W. W. JOHNSON, Annapolis, Md.; B — C. K. WEAD, Ann Arbor, Mich.; C — J. W. LANGLEY, Ann Arbor, Mich.; D — A. J. DUBOIS, New Haven, Conn.; E — ALEXIS A. JULIEN, New York, N. Y.; F — S. A. FORBES, Normal, Ill.; G — CARL SEILER, Philadelphia, Pa.; H — G. H. PERKINS, Burlington, Vt.; I — JOSEPH CUMMINGS, Evanston, Ill.

Treasurer — WM. LILLY of Mauch Chunk, Pa.

The whole list was elected amidst considerable enthusiasm.

Dr. T. STERRY HUNT made a statement in reference to the International Geological Committee, of which Professors HALL, SELWYN, LESLEY, and himself had been appointed representatives from North America. Several months ago a report was prepared by them. The work was not yet completed, and, on the recommendation of the STANDING COMMITTEE, he moved that the Committee be continued. The motion was seconded by Professor HALL, and the Committee was continued.

The PERMANENT SECRETARY made a number of announcements, and the PRESIDENT gave some particulars regarding the excursions for the day. The Session then adjourned to meet in Sections.

Sections E, H, and I finished their work, and finally adjourned at the close of the morning session.

In the afternoon the members were taken on an excursion around the harbor to the Victoria Bridge and to the Grand Trunk workshop, returning soon after six o'clock, well pleased with their trip.

At 8.30 P. M. the concluding General Session was held in the William Molson Hall, when there was a large attendance of the members of the Association and the citizens of Montreal.

President DAWSON said they had now arrived at the closing exercises of the meeting, and that he would call upon the Permanent Secretary, Professor PUTNAM, to make some remarks with reference to his latest work, the programme of the Association.

In reply, the PERMANENT SECRETARY stated that this was the largest meeting the Association had ever held, with the exception of that at Boston two years ago, when sixty more names were entered on the register. During the present meeting 937 names were recorded on the Association register. Of these, 20 were invited guests and their ladies, from England, Ireland, Hungary, Austria, France and Russia; 579 members and 338 representatives of families of members were in attendance from the following places: Montreal, 90, other parts of the Dominion of Canada, 44; New York, 199; Massachusetts, 158; Ohio, 74; District of Columbia, 60; Pennsylvania, 43; New Jersey, 38; Connecticut, 30; Illinois, 22; Missouri, 22; Michigan, 18; Vermont, 16; Maryland, 15; Rhode Island, 14; Maine, 13; New Hampshire, 12; Indiana, 10; Florida, 5; South Carolina, 4; Virginia, 4; Colorado, 3; Minnesota, 3; Nebraska, 3; California, 2; Tennessee, 2; Mississippi, 2; North Carolina, 2; Louisiana, 2; Wisconsin, 2; and Alabama, Kentucky, West Virginia, Kansas and Iowa, one each; total, 937. Three hundred and twenty-five new members had been elected besides the members of the Local Committee who wished to accept membership in the Association. Two hundred and fifty-four papers had been sent in, which, with the exception of twelve, had been called up in the nine sections, that had been in session during the greater part of the week, and nine addresses and two lectures had been delivered.

The following resolutions were passed unanimously:

Prof. E. D. COPE, of Philadelphia, moved the following resolution:—

That the Local Committee of the Association have won our heartfelt esteem for the admirable efficiency with which they have planned and carried out the arrangements for our comfort. Especial mention should be made of the beautiful and complete "Handbook to the City of Montreal," prepared for the occasion by Mr. S. E. DAWSON, which has been a constant guide to us all; of the labors of the Railway Committee, Lodging Committee, the Excursion Committee, the Finance Committee, the Printing Committee, and the Reception Committee.

In moving this resolution, Professor COPE enumerated the different duties performed by the various committees, and expressed the hope that

they had received some return for the great sacrifice which they had been called upon to make.

Dr. E. B. ELLIOTT, of Washington, moved the following:—

That the American Association for the Advancement of Science, deeply sensible of the kindness which they have received in Montreal, hereby declare their thanks to the hotels, the Diocesan Theological College and private citizens for hospitality, and to the ladies for the elegant garden parties and other courtesies extended to the members and their families.

In moving this resolution Dr. ELLIOTT said he wished that he felt able to move it in a fitting manner. His duties as Vice President of one of the Sections had occupied the most of his time, and he had not had much leisure to take advantage of many of these entertainments. He referred to the hospitality extended to the members of the Association in this city twenty-five years ago, which he had no doubt was prompted by their love of science, and said he felt that the newspapers of Montreal were educating the people to a high standard in this direction, and their example might be profitably followed even by some of the prominent newspapers of the United States. He concluded by referring to the very large accession of members which the Association had received at this meeting.

Professor COX moved the following resolution:—

That the American Association hereby extends its heartfelt thanks to the Corporation and Faculty of the McGill University, of the Presbyterian Theological College, and the Society of Natural History, for commodious quarters during the meeting, and for the consideration with which every want has been anticipated and gratified.

In offering this resolution Professor COX said he considered that it would be fully sustained by all the members. They had seldom been provided with more extensive, commodious or convenient rooms, and he was sure they would long remember the edifices of this University, its splendid library, museum and beautiful grounds.

Prof. WILLIAM SAUNDERS, of London, Ont., moved the following:—

That the thanks of the American Association are due and are most heartily tendered to the numerous railways of the United States and the Dominion, and to the several steamship companies, for their courtesy in affording members excellent facilities for attendance at this meeting. We especially acknowledge our obligations to the Grand Trunk Railway Company for the excursion to Lachine, and for the freedom of that monument of engineering skill, the Victoria Bridge. We also thank the Harbor Commissioners for the excursion of to-day.

Prof. Saunders in his remarks said that the subject of transportation was in one sense a dull one, and had nothing very transporting about it. The Companies had so carefully conducted their arrangements that the baggage as well as the persons of members had been well cared for.

Professor LEEDS moved the following resolution:—

That the cordial thanks of this Association are due and tendered to the Telegraph and Telephone Companies for their great liberality in extending the free use of their wires to the members and fellows during the meeting.

In offering this resolution, Prof. LEEDS said he did so with a full sense of the great obligation they were under to these companies for the aid they had given. But as the Association for the Advancement of Science he thought these companies in their turn owed them something, as it had been the mother of electricity, and one of its members shared with Morse the honor of the discovery of the electric telegraph,—Joseph Henry. (Applause.) And since that time the Association had given much thought and care to the subject of electricity, as evinced by the valuable contributions from Edison and other great electricians, and the recent valuable lecture by Prof. Alex. Graham Bell. He, therefore, thought they could say that the privileges granted by the companies were in one sense a due concession to science.

Professor PUTNAM remarked that for two months previous to the meeting the telegraph company had given free transmission of messages to himself and the Secretary of the Local Committee in Montreal.

Dr. T. STERRY HUNT, of Montreal, moved:

That the members of the Association and their families will ever hold in grateful remembrance the generosity of the Richelieu & Ontario Navigation Company and of the North Shore Railway for the delightful excursions to Quebec, and the Mayor, Local Committee and citizens of that deeply interesting spot, for the harbor excursion and the bounteous collation provided for the enjoyment of their guests.

The mover referred to the incidents of the trip and the pleasure enjoyed by all those who attended the visit to Dufferin Terrace, to the ancient Citadel, Laval University, the Harbor, etc. He considered that all those who attended the excursion would need have nothing said to them to make them support the resolution. He called upon Dr. HINGSTON, as Chairman of the Conveyance Committee, to add a few words of thanks to the railway and steamboat authorities.

Dr. HINGSTON made a few remarks, in the course of which he said that the President, Dr. HUNT and himself were surviving members of the Local Committee who entertained the Association in this city twenty-five years ago. If Darwin or any of his disciples were present he would say it was another illustration of the survival of the fittest. (Laughter.) He expressed their thanks to the railway authorities for the readiness with which they had responded to their appeals, and did everything in their power to add to the comfort and convenience of the members of the Association.

The PRESIDENT, Dr. DAWSON, remarked that the members of the Association felt very much indebted to the citizens of Quebec for the aid they had given in making this meeting an agreeable one.

Dr. JOHN RAE moved the following resolution:—

That the thanks of the Association are most cordially tendered to the Canadian Pacific Railway for the special train to Ottawa, and to the Mayor, Local Committee and citizens of Ottawa, for the delightful view of the natural beauties, industries and public buildings of the place, for their elegant repast and for their kind words they were pleased to bestow upon the Association.

Dr. RAE said that all those who went to Ottawa and saw its beautiful buildings would agree with him that it was one of the most charming days they spent. He spoke of the beauty of the Parliament buildings, and the universal admiration with which they were regarded.

Prof. JAMES HALL, in supporting the motion, said that no one who had seen the library of Parliament at Ottawa could fail to express their admiration. It had no equal in the United States. The Museum at Ottawa was also a valuable institution, and it must always be the resort of scientific men. For this reason they should foster it. He, the speaker, had advocated the holding of the first meeting of the Association at Montreal twenty-five years ago, and had been largely instrumental in having it held here. He had many pleasant reminiscences of that meeting, and would always remember Montreal.

The Rev. Dr. HOVEY moved the following:

That the thanks of the American Association are due to the gentlemen of the local press, and the agents of papers in other cities, for the untiring pains which they have taken in reporting the proceedings of the Montreal meeting.

In moving this resolution, Dr. HOVEY said if it were not for the gentlemen of the press the deliberations of the members would not be so widely known as they are. He enumerated the various city papers to which they were indebted, as well as those of Boston, New York and Chicago, and also added that the proceedings of the Montreal meeting were, he considered, better reported than any previous one.

Dr. HAUGHTON of Dublin offered the following:

The American Association is especially honored at this Montreal meeting by the presence of so large a number of eminent visitors from abroad, thereby conferring an international character upon this assemblage on Canadian soil.

He said that a task had been assigned to him which no one but an Irishman could perform. He had been required to propose a vote of thanks to himself and then respond to it. (Laughter.) After returning thanks on behalf of the visitors for the courtesies extended to them by the Association, he said the only way in which he could express his opinion of the hospitality of which they had been the recipients, was by saying that if his mother had not been an Irishwoman he could have wished she had been a Canadian. [Applause.]

Prof. O. T. MASON, with a few humorous remarks, moved:

That a vote of thanks be returned to the Southeastern Railway Company for the contemplated excursion to Newport and Lake Memphremagog.

Dr. ALEX. WINCHELL moved:

That the thanks of the Association be given to the officers of McGill University, Mechanics' Institute, Art Association, Natural History Society, the Montreal and Amateur Athletic Association, Numismatic Society, Young Men's Christian Association, and the Merchants' Exchange for invitations extended to members of the Association.

Dr. WINCHELL, in moving it, spoke in high terms of the Art Gallery and the Peter Redpath Museum, and the high educational value of such collections to the citizenship of the community at large.

After the passage of the several resolutions, President DAWSON brought the proceedings to a close with some observations in regard to the object of the gathering just brought to a termination. It was very gratifying, he said, to know that this has been one of the most successful meetings of the Association, not only in regard to attendance, but in respect to the papers read and the practical benefits which might be expected to follow. He hoped all would carry away something which would make them wiser and better. He also hoped that they would carry away with them pleasant memories and a stimulus and encouragement which would lead them to greater achievements in the future.

The meeting then adjourned to meet at Minneapolis on the third Wednesday in August in next year.

ON THURSDAY MORNING

A party of nearly four hundred, composed of members and their ladies, availed themselves of the liberality of the SOUTHEASTERN RAILWAY and took part in the complimentary excursion to the little town of Newport and the picturesque lake Memphremagog. The weather was propitious and the party reached their destination at 12.30, where after partaking of an excellent dinner they embarked on the steamer "Lady of the Lake," and enjoyed a magnificent sail of about seventeen miles up, passing many points of interest and returning to Newport in time to take the 5.15 train to Montreal where the members finally separated.

WM. SAUNDERS,
General Secretary.

REPORT OF THE PERMANENT SECRETARY.

By holding a meeting for the second time beyond the limits of the United States, the Association has shown that its name of American is to be received in the widest sense.

A quarter of a century ago its eleventh meeting was held in Montreal, and again it was welcomed to the city for its thirty-first. In 1857 there were 351 names entered on the register of persons in attendance, and about 250 new members were elected; in 1882 there were 937 names registered and over 300 new members were elected. In 1857, 118 papers were read before two sections and one subsection, the regular presidential address and two memorial addresses were delivered; in 1882, 242 papers were presented before nine sections, and in addition to the presidential address there were seven by vice-presidents, also one special address and two evening lectures. In the volume of the proceedings relating to the eleventh meeting there are given the names of 946 members; in the present volume there are 1922. These few comparative statements show the growth of the Association in twenty-five years, and in this connection it must be remembered that during the civil war the meetings were suspended for five years.

Of the 255 papers entered on the list of titles received, one came after section D, to which it belonged, had adjourned; three were withdrawn by their authors, and eight were declined for various reasons. The paper by Mr. Meehan was given as an address before the biological section in accordance with the vote of the former section B at the Cincinnati meeting, and Mr. Hovey's paper on cave scenery was delivered in the theatre of the Redpath Museum on the evening of the formal opening of the Museum. Of the remaining 241 papers, 18 were referred to section A, 37 to B, 29 to C, 9 to D, 45 to E, 40 to F, 16 to G, 32 to H, and 15 to I.

The present volume contains the address delivered by Professor Hall, as retiring president of the Montreal meeting of 1857, now for the first time printed. The presidential address of Prof. Brush, and those of seven vice-presidents, the special address of Mr. Meehan, an abstract of Dr. Carpenter's lecture, and 124 papers either in full or by more or less extended abstracts as follows: Section A, 16; B, 13; C, 14; D, 7; E, 25; F, 25; G, 10; H, 10; I, 4. One hundred and seventeen other papers are mentioned by title simply, either from their authors failing to send abstracts for publications, or, in a few instances, in accordance with the votes of the standing committee that they should so appear. The delay in printing the volume has been due to a combination of causes for which neither the secretary nor the printers are responsible.

Of the 87 members elected fellows during the meeting, one has resigned his membership, one has declined, four have not yet answered the notices sent them, and 81 have accepted.

Of the new members elected, fifteen were invited foreign guests, twenty-two have declined, forty-eight have not yet been heard from, and 288 have paid in full, and their names are entered on the roll.

Seven members and fellows have become life-members since the report given in the Cincinnati volume.

For a statement relative to the 937 members and others registered at the meeting, I refer to the paragraph on page 635 of this volume.

Two thousand and fifteen copies of the Cincinnati volume of proceedings were received from the printers, and 1497 were sent to members entitled to receive them, 23 have been sold, and 177 distributed in accordance with votes of the Standing Committee. Of the preceding volumes, 77 have been sent to members entitled to them, 51 sold, and 19 have been distributed by vote of the Standing Committee. Four of the early volumes have been purchased and two have been received as a gift.

The Nashville volume was reprinted and stereotyped during the year, thanks to the liberality of General Lilly, and 253 copies were received from the printer. The St. Louis volume will be reprinted during the present year by the liberal gift of a member. After this is done there will be left on hand several hundred dollars which will be reserved for reprinting the second volume as soon as the required amount is secured.

Since the report given in the Cincinnati volume, under date of April 26, notices of the decease of 39 members and fellows have been received, among whom are three past-presidents, including the only Honorary Fellow of the Association; the former Treasurer, a former General Secretary, and four original members. Of these the names of thirty-two were read at Montreal and are recorded on pages 618–19 of this volume.

During the same period the names of 67 members and fellows have been taken from the list in consequence of resignations or from their being over two years in arrears in their assessments. In the same time 24 names have been restored to the list. Seven members elected at Cincinnati have accepted during the past year, in addition to the 319 mentioned in the last report.

The total number of members and fellows at the present date is 1920, a net gain of 221 since the list was printed in the Cincinnati volume.

For a statement of the receipts and expenses for the year covering the Cincinnati meeting and ending just previous to the Montreal meeting, I refer to the cash account on the following pages.

The invested funds of the Association are as follows:

General Fund, $84 67, int. to Aug. 1, '83, $4 23, . . .	$88 90	
Life Membership Fund, $1900 54, int. to Aug. 1, $73 00,	1973 54	
		$2062 45

The subscription fund for reprinting stands as follows:

June 10, 1881,	Gift of A. Schaffranck, ...	5 00	
Aug. 16, 1881,	Mrs. H. Herrman, ...	100 00	
	G. J. Brush, ...	20 00	
	William Bross, ...	20 00	
	William Lilly, ...	10 00	
	Thomas Meehan, ...	5 00	
	George Holley, ...	5 00	
	Robert Peters, ...	5 00	
	L. H. Trowbridge, ...	5 00	
	W. LeConte Stevens, ...	5 00	
	F. P. Dewey, ...	5 00	
	Alfred Springer, ...	5 00	
	J. D. Cox, ...	5 00	
	S. F. Peckham, ...	2 00	
	J. B. Killebrew, ...	2 00	
	F. W. Clarke, ...	2 00	
	W. H. Walmsley, ...	1 00	
	Several unknown contributors,	6 00	
April 10, 1882,	William Lilly, ...	1000 00	
July 3, 1882,	Geo. L. Vose, ...	1 00	
August 23, 1883,	A Member, ...	850 00	
Interest to Aug. 14, 1882,	. . . . .	8 16	
Interest to Aug. 1, 1883,	. . . . .	46 30	
			$2113 46

Cost of reprinting and stereotyping Nashville volume,	$857 94	
Balance on hand, ...	1255 52	
		$2113 46

Respectfully submitted to the Association,

F. W. PUTNAM, *Permanent Secretary.*

Salem, Mass., July 27, 1883.

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

1881-2.

To Assessments previous to the 30th Meeting . .	$408 00		
" Cincinnati, " " . .	3,315 00		
" Montreal, 31st " . .	1,359 00		
			$5,082 00
Entrance fees Boston Meeting	15 00		
" " Cincinnati "	1,485 00		
" " Montreal "	330 00		
Fellowship fees	112 00		
			1,942 00
Sales of Publications and Binding	334 76		
Advertisements on covers	111 00		
Illustrations	3 00		
Postage 1.18; sale of old paper .55	1 73		
			450 49
Life Membership commutations (Intr. 6.00) .	256 00		
Representing fund, subscriptions (Intr. 8.15) .	212 15		
William Lilly, Patron	1,000 00		
			1,468 16

IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE. Cr.

1881-2.

By 2015 copies Proceedings, Vol. 30, Cincinnati Meeting:—			
Composition, stereotyping, and boxes for plates	$1,129 40		
Illustrations	22 49		
Paper and press-work	921 40		
Covers	20 00		
Binding 1,962 in paper 470.88; 40 in cloth 20.00	490 88		
" 15 in half-morocco 15.00; 25 covers 5.00	20 00		
Printing wrappers and wrapping	18 00		
Distribution by mail and express	229 30		
Extras of Addresses and Reports	38 19		
			$2,889 66
Vol. 29, Illustrations 2.40; Addresses 9.50	11 90		
Distribution, balance of postage and express	120 17		
			132 07
Expenses of Cincinnati Meeting			488 96
Publications bought, and binding			57 62
Extras of Addresses Montreal volume			15 41
General expenses:—			
Rent of office to July 1, 1882	100 00		
Janitor to Aug. 1, 1882	102 00		
Fuel 40.50, Express 46.40	86 90		
Minor expenses, stationery, office furniture, etc.	25 45		
Subscription to Postal Guide	1 50		
Postage and envelopes 286.31, P. O. box 8.00	294 31		
Printing circulars, blanks, cards, etc.	260 76		
Extra clerk hire	33 25		
			904 17
Balance paid Perm. Sec'y, due last acct.			773 03
Salary of Assistant Secretary	400 00		
" " Permanent "	1,000 00		1,400 00
Life Membership Fund 250.00, Intr. 6.00	256 00		
Reprinting Fund, including 5.00 received in 1880	365 62		
" and stereotyping Vol. 26 (250 copies)	851 54		1,473 16
Balance to new account			808.57
			$8,942 65

the same correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor.*

INDEX.

PAGE

Acids....281, 282, 283
Act of Incorporation.... xxii
Address of Retiring President.... 1, 622
Address of Retiring President delivered in 1857.... 29
——— — Vice President, Section A.... 77
——— — — — ——— Section B.... 127
——— — — — ——— Section C.... 220
——— — — — ——— Section D.... 295
——— — — — ——— Section F.... 423
——— — — — ——— Section G.... 499
——— — — — ——— Section H.... 531
Aërial navigation.... 318
Agricultural statistics.... 599
Air and ocean, Currents of.... 367
Aletia xylina, Hibernation of.... 468
American continent, Geological history of.... 29
Amylose.... 285
Ancient customs, Unnoticed affinities.... 569
Andrews, L. W., Constitution of Benzole.... 286
Anemometer, Mean direction integrating.... 147
Archæological discoveries in Vermont.... 592
Art Association.... 618
Arteries, Morphology of.... 488
Athletic Association.... 623, 627
Ayres, Brown, Title of paper.... 226

Baker, Frank, Morphology of Arteries.... 488
——, T. R., Title of paper.... 321
Balloon, Future of as means of aërial travel.... 319
Baric hydrate, Action of.... 282
Barker, F., Secondary batteries.... 207
——— —, to prepare Memoir on W. B. Rogers.... 627
Bartlett, J. R., Deep-sea soundings and temperatures in the Gulf Stream off the Atlantic coast, taken under the direction of the U. S. Coast and Geodetic Survey.... 349
——— — —, Siemens' electrical deep-sea thermometer.... 221
Bateman. F. F., Title of paper.... 321
Beal, W. J., Motions of roots and radicles of Indian corn and of beans.... 466
Beams, spruce, strength of.... 311
Beans and Indian Corn, roots and radicles of,.... 466
Bell, A. G., Electrical experiments to determine the location of the bullet in the body of the late President Garfield; and upon a successful form of induction balance for the painless detection of metallic masses in the human body.... 151

	PAGE
Bell, A. G., Paper by	624
——— — —, Proposed method of producing artificial respiration by means of a vacuum jacket	224
———, A. M., Lecture by	626
Benzole, Constitution of	286
Bessey, C. E., Action of frost upon leaf-cells	464
Bickmore, A. S., Title of paper	612
Binocular union of spectral images	149
Bishop of Montreal, Invocation by	613
Bituminous matter, Source of	573
Blackwell, A. B., Cross heredity from sex to sex	492
Blake, C. J., Title of paper	496
Blood corpuscles in bone manure, Development	512
———, Microcytes, in, Probable origin	513
———, Third corpuscular element in	511
Bolton, H. C., Address, Vice President, Section C	229
——— ——— —, Application of organic acids to the examination of minerals	271
——— ——— —, Title of paper	232
Bowditch, H. P., Illusions of motion	489
Bowers, V. K., Title of paper	505
Brains, Preparation of, by Giacomini's method	491
Brewer, W. H., Apparent size of magnified objects	139
Britton, N. L., Post-tertiary deposit containing impressions of leaves, in Cumberland County, New Jersey	357
Brooks, W. K., Titles of papers	496
Bross, William, Title of paper	629
Brush, G. J., Address of	1
——— ———, Opens the meeting	613
Buckhout, Wm. A., Gall-mites, Phytoptus	473
Bunsen flame, Modifications of the spectrum of sodium vapor in	218
Burrill, J. T., Some vegetable poisons	515

Caffeine in tea	287
Calcite, Crystals of	429
Caldwell, G. C., Pemberton's method for the [illegible] of phosphoric acid	220
Canadian Devonian, Fishes in	353
Canadian Paleozoic floras	423
[illegible]	253
Carmichael, [illegible]	235
——— ———, Title of [illegible]	232
Carpenter, W. B., Lecture [illegible]	630
——— ——— ———, Remarks [illegible]	627
Cast-iron [illegible]	442
[illegible]	611
Chairs, Experimental [illegible]	229
Chase, P. V., [illegible]	248
——— ——— ———, Title of [illegible]	235
[illegible]	626, 627
——— [illegible]	627
[illegible]	573
[illegible]	245
[illegible]	477
[illegible]	502
[illegible]	227
Chaplin, J. W., [illegible]	626, 628

PAGE

Clematis, Revision of 463
Climates 367
Colorado, Silicified stumps of 398
Committee, Local, Minneapolis Meeting xv
———— ———, Montreal Meeting xi
Committees, Reports of 625, 626
Committees, Special xvi
Communications, Titles, ... 124, 225, 292, 321, 419, 495, 528, 594, 612
Compositæ, Achenial hairs and fibres 463
Constitution xxiii
————, Proposed amendment to 633
Cook, E. H., Carbon dioxide as a constituent of the atmosphere 276
——— — —, Simple laboratory device 281
———, G. H., Change of relative level of the Ocean and the Uplands of the eastern coast of North America 400
Cope, E. D., Classification of the Ungulata 477
——— ———, Fauna of the Puerco Eocene 479
Crampton, C. A., Estimation of dextrose in solid commercial starch sugar by loss of rotatory power on solution 285
Crevier, J. A., Title of paper 496
Crosby, W. O., Classification and origin of joint structures 409
Cross and Crucifix 572
Cryptobranchus, Habits of 482
Currant saw-fly, Egg-parasite of 471
Currents of air and Ocean 367
Currier, J. McNab, Title of paper 595
Cylinders, Strength of 317

Dabney, C. W., jr., Titles of papers 292
Dall, W. H., Address, Vice President, Section F 423
Darwin, Memorial to 625
Darwin's theory of the earth-moon system 89
Davenport tablets 584
Dawson, J. W., Comparative view of the successive palæozoic floras of Canada 415
——— — —, Remarks by 616, 628, 635, 639
——— — —, Remarks on taking the Chair 613
Deceased Members lxxviii
———— ————, Notices of 618
Deep-sea soundings 349
De Hass, Wills, Titles of papers 504
Devonian fishes in Canada 353
Dextrine 285
Dextrose 283
Dhegiha, Kinship system and marriage laws 579
Dodge, J. R., Statistics in Agriculture 599
Dolbear, A. E., Titles of papers 225, 226
Dorsey, J. O., Kinship system and marriage laws of the Dhegiha, illustrated by charts 579
————, Titles of papers 594, 612
Douglass, A. E., Find of ceremonial weapons in a Florida mound, with brief notice of other mounds in that state 585
Dudley, William, Title of paper 292
Dwight, W. B., Recent investigations and palæontological discoveries in the Wappinger limestone of Dutchess and neighboring counties, New York state 384

	PAGE
Earth moon system, New views of Darwins' theory	89
Eastern coast of North America, Change of level	400
Ebbly, H. T., Radiant heat an exception to the second law of thermodynamics	225
Edmunds, J. R., Title of paper	613
Edwards, W. H., Titles of papers	496
Electrical experiments to determine the location of the bullet in the body of President Garfield	151
Electric spark, Vision by the light of	148
Elliott, A. H., Titles of papers	298
H. H., Titles of papers	613
Ellis, W. H., Some tea analyses, with special reference to the determination of caffeine	287
Elm leaf beetle	473
Elsberg, Louis, Plant "cells" and living matter	511
Elections	625, 635, 639
Fauna of the Puerto Rican	49
Feldspars, Determination of	257
Fellows	lxiv
elected	609
Fencing Wash basin	535
Fishes, Distribution of Canada	553
Flies, [illegible]	580
, Table of papers	586
Forest, North American	449
Fox, [illegible]	435
Franklin, [illegible]	555
[illegible]	570
[illegible]	577
[illegible]	474
[illegible]	579
[illegible]	167
[illegible]	570
[illegible]	573
[illegible]	608
[illegible]	581
[illegible]	573
[illegible]	611
[illegible]	581
[illegible]	581
[illegible]	581

PAGE

Hall, Asaph, Parallax of α Lyræ and 61 Cygni 93
—— E. H., Title of paper 226
—— G. S., Illusions of motion 489
—— James, Address of, delivered in 1857 29
———— ———, Titles of papers 419
Hamilton, W. E., New method of graphically expressing any cyclical fact in Meteorology 121
Hanaman, C. E., Filtering wash-bottle especially adapted to the use of the histologist 525
Hare, R. B., Association of crystals of quartz and calcite in parallel position 419
Harkness, William, Address, Vice-President, Section A 77
Hart, C. P., Arithmetic of chords 105
Hastings, C. S., Modifications of the spectrum of sodium vapor in a Bunsen flame 218
Haughton, Samuel, New views of George H. Darwin's theory of the evolution of the earth-moon system, considered as to its bearing on the question of the duration of geological time 89
Heredity, Cross, from sex to sex 492
Heteropora, Recent species from Strait of Juan de Fuca 481
Hingston, W. H., Title of paper 595
Histology of uterine fibroid tumors 513
Hitchcock, C. H., Glacial flood of the Connecticut River Valley 325
Honorary Fellow lxiv
Hough, F. B., Titles of papers 612
Hovey, H. C., Paper by 623
——, —— —, Subterranean map-making 345
Hoy, P. R., Titles of papers 594
Hunt, G. J., Address 622
——, T. S., Address of welcome 613
———— — —, Chairman of Local Com., Notices by 618, 622
———— — —, Remarks by 618, 634
———— — —, Titles of papers 419
Hurlbert, J. B., Currents of air and ocean in connection with climates, regions of summer rains and summer droughts 367
Hyatt, James, Title of paper 612

Iles, George, Title of paper 225
Indian corn and beans, roots and radicles of 406
Indian migrations as evidenced by language 578
Indicator attachment for high speeds 316
Induction balance 151
Injection apparatus, New form of 521
Insecticides 469
International Geological Committee 634
———————— Scientific Association 624
———————— Standard Time, Committee 634

James, J. F., Revision of the genus Clematis of the United States 463
Johnson, W. W., Circular Coördinates and Complex Anharmonic Ratios 106
———————— — —, Inverse elliptic functions and the imaginary period 107
Joint-cracks, Origin of 417
Joint structures, Classification and origin 409
Julien, A. A., Titles of papers 419, 420

PAGE

Kinnicutt, L. P., Action of water at 100° C. on the B phenyltribrompropionic acid..... 281
Kinship system and marriage laws of the Dhegiha..... 579
Kitsee, Isidore, Title of paper..... 293
Konig, Rudolph, Title of paper..... 295
Kowalevsky, D. W., Title of paper..... 490

Laboratory device..... 281
Lake Ontario, Ancient terraces of..... 359
Langley, S. P., Title of paper..... 225
Lanza, Gaetano, Transverse strength of large spruce beams..... 311
Lawes, J. B., Title of paper..... 293
Laws and Gilbert, Vote in relation to paper by..... 633
Leaf cells, Action of frost..... 464
Leaves in the Post-tertiary of New Jersey..... 357
Leeds, A. R., Preliminary notice of a new organic base..... 284
Lepidoptera, New Sexual character in pupæ of..... 470
L'Etoile, J. de, Aërial navigation..... 318
Level, New form of..... 102
Lewis, H. C., Great Terminal Moraine across Pennsylvania..... 389
Libbey, William, jr., New form of constant pressure injection apparatus..... 521
Lilly, William, Made patron..... 618
Lintner, J. A., Egg-parasite of the Currant saw-fly, Nematus ventricosus..... 471
——— ——, New sexual character in the Pupæ of some Lepidoptera..... 470
Local Committee, Reception by..... 623
Lockwood, Samuel, Mastodon Americanus in a beaver meadow, New York... 365
Lynch, W. H., Future of the balloon as a practical means of aërial travel. .. 319

Mabery, C. F., Action of baric hydrate on chlorobrompropionic acid..... 288
——— ——, Certain saturated acrylic and propionic acids..... 283
——— ——, Products of the distillation of wool at low temperatures..... 294
Macfarlane, A., Action of Sulphuric acid on Cymene..... 463
——— ——, Observations on the Electrical forces of Gymnotus [illegible]..... 472
Magnetic ores in Ontario..... 413
Malone..... 285
Marsh's Pleistocene [illegible]..... 623
Marvy, R. W. [illegible]..... 513
——— ——, Pleistocene [illegible] a study of [illegible] and [illegible]..... 488
[illegible]..... 579
[illegible]..... 587
[illegible]..... 384
[illegible]..... 385
[illegible]..... 470
[illegible]..... 486
[illegible]..... 457
[illegible]..... xx
——— [illegible]..... xx
[illegible]..... xxx
[illegible]..... 15
——— ——— [illegible]..... [illegible]
——— ——— [illegible]..... [illegible]
——— ——— [illegible]..... 29
[illegible]..... 288
[illegible]..... 623

PAGE
Metal, Linear elasticity 118
Meteorology, Cyclical fact in 121
Microscopic Club 623, 626
Mills, T. W., Title of paper 496
Minneapolis, Invitation from 627
——— Meeting, Local Committee xv
——— ——— Officers xiii
——— ——— ——— elected for 634
——— ——— Standing Committee xiv
———, Voted to hold meeting at 628
Mineralogists, List of 21
Mineralogy, Sketch of the progress of 1
Minerals, List of, in the United States and Canada 21
Mixter, C. S., Facts relative to the U. S. census of 1880 611
Montreal Meeting, Local Committee xi
——— ——— Officers ix
——— ——— Standing Committee x
Moraine, Terminal, across Pennsylvania 389
Motion, Illusions of 489
Mounds in Florida 585
Mount Atlas and its traditions 559

Nematus ventricosus, Egg-parasite of 471
Newberry, J. S., Titles of papers 420
Newton, Sir Isaac, Corrections of problem of 109
New York, Undulations of rock-masses 412
—— ——— Wappinger limestone 384
Niagara formation of Canada, Graptolites in 363
Nichols, E. L., Duration of color-impressions upon the retina 146
North America, Change of level of eastern coast 400
North America, Flora of 449

Objects, magnified, apparent size of 139
Officers elected for Minneapolis Meeting 634
——— Minneapolis Meeting xiii
——— Montreal Meeting ix
Ogalalla Sioux, Sun dance 580
Ohio, Black Shales, source of Bituminous matter in 373
Oliver, J. E., Method of finding the law of linear elasticity in a metal 118
——— — —, On the law of distribution for certain plant-numbers 117
Ontario, Magnetic ore 416
Order of Proceedings in organizing Meeting xxx
Organic acids, Application of 271
——— base, Notice of 284
——— compounds in their relations to life 493
Organisms, Classification of 494
Orton, Edward, Source of the bituminous matter in the Black Shales of Ohio 373
——— ———, Title of paper 420
Osborn, H. F., Title of paper 496
Osler, William, Demonstration of a series of brains prepared by Giacomini's
——— ———, method 491
——— ———, Development of blood corpuscles in the bone-marrow 512
——— ———, Microcytes in the blood and their probable origin 512
——— ———, Third corpuscular element in the blood 511
——— ———, Title of paper 528

PAGE

Ottawa Excursion..... 65
Owen, Richard, Contribution to seismology..... 329
——— ———, Law of fracture, or jissuring, applied to inorganic and organic matter..... 337

Paleozoic flora of Canada..... 415
Palmer, G. M., Action of water at 150° C. on the Potsdam sandstone and 281
Parallax of a Lyrae and 61 Cygni 98
Parkhurst, H. M., Bell attachment for telescope tubes..... 111
Parsons..... xxxi
Pendulum's motion, on the relative motion of a, one point 29
Penhallow, D. P., Structural and physiological peculiarities incident to the
——— ———, Leaves of Pinus..... 529
——— ———, Title of paper..... 235
Perley, Sir A. M., Notice of 289
Perkins, G. H., Recent archæological discoveries in Vermont..... 388
——— ———, Title of paper..... 394
——— ———, Wooden implements of Vermont..... 388
Permanent Secretary, Cast Address 66
——— ———, Notices by..... 67, 68, 69, 70, 71, 72, 73, 74, 75
——— ———, Report..... 49
Petroleum, Value as fuel..... 69
Pierce, J. E., Some [illegible] between ancient customs in America and [illegible]..... 388
Phosphoric acid, Volumetric determination..... 289
Physiology..... 63
Pickering, E. C., Determination of the Parallax of Alpha Aquilæ by 97
Plankton, Investigations of, in Vineyard 68
Plankton, Report on, during the [illegible] 111
Plankton, Law of, in the Atlantic 117
Population in United States 121
Precipitation, Investigations of, in New 85
Precipitation, Relation to Lake Levels..... 132
Precipitation, Progress in 1
Problems, Mechanical, Illustrative of 63
Proceedings, Subscriptions for, during vacation 62
Proceedings, Publication of 67
Proceedings, Distribution of 67
Proceedings, Synopsis of 9
Publication, Report of Committee on 8
——— ———, Expenses of 38

Quartz, Crystallized 43
Quebec, Visit to 65

Rainfall, Variations in 68
Rainfall, Report on 16
Rainfall, [illegible] 38
Rainfall, [illegible] 38
Rainfall, [illegible] 68
——— ———, [illegible] 68
——— ———, [illegible] 68
——— ———, [illegible] 68
——— ———, [illegible] 68
——— ———, [illegible] 68
——— ———, [illegible] 68

PAGE
Records of Science 624
Redpath Museum, Opening of 623
Religions, American, Chief deities 573
Report of General Secretary 613
Report of Permanent Secretary 640
Resolutions of Thanks 635
Respiration, Artificial 224
Retina, Durations of color-impressions 146
Riley, C. V., Emulsions of petroleum and their values as insecticides 469
——— — —, Hibernation of Aletia xylina (Say), in the United States, a settled
——— — —, fact 468
——— — —, Observations of the fertilization of Yucca, and on structural and
anatomical peculiarities in Pronuba and Prodoxus 467
Robinson, F. C., Certain substituted acrylic and propionic acids 283
——— S. W., Title of paper 225
Rock-masses, Undulations of 412
Rockwell, C. H., Title of paper 124
Rogers, W. A., Method of reducing different catalogues of stars to a homoge-
——— — —, neous system 99
——— — —, Performance of a new form of level invented by John Clark 102
——— — —, Titles of papers 226, 528
——— W. B., Memoir to be prepared 627
——— — —, Resolutions on 524
Rowland, H. A., Title of paper 226
Rust, H. N., Davenport tablets 584
——— — —, Title of paper 504

Sands, Fred., Title of paper 292
Saunders, William, Mouth of the larva of Chrysopa 477
——— ——— Report of General Secretary 613
Screws of increasing pitch, Method of cutting 314
Seaman, W. H., Title of paper 496
Secondary batteries 207
Sections, Officers and committees of 620, 623, 634
Seismology, Contribution to 339
Siemens' electrical deep-sea thermometer 221
Silicified stumps of Colorado 398
Silliman, W. A., Title of paper 496
Siphonotreta Scotica 356
Smiley, C. W., Exhibition of some statistics of college men 607
——— — —, Methods and results of collecting statistical matter by mail .. 601
Smith, E. A., Titles of papers 595
Smock, J. C., Title of paper 420
Solar energy, Conservation of 108
Southeastern Railway, Excursion given by 639
Spencer, J. W., Occurrence of Graptolites in the Niagara formation of
——— — —, Canada 363
——— — —, Terraces and beaches about Lake Ontario 359
Standard Time, Report from Section I 633
Standing Committee, Minneapolis Meeting xiv
——— ———, Montreal Meeting x
Starch-sugar 285
Stars, Reducing different catalogues 99
Statistical matter, Methods and results of collecting by mail 601
Statistics, Agricultural 599
——— of college men 607
Steam, Limit to expansion 307